THE MANKIND QUARTERLY

VOLUME XXXIX No. 4 Summer 1999

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Apollo the Wolf-god

Daniel E. Gershenson

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Cover Picture: Artist's impression of a Neanderthal, shaven with close hair-cut, and attired in mid-20th century clothing, based on a reconstruction of the fossil skull found at La Chapelle aux Saints. [*See Article:* Social Organization, Group Conflict, and the Demise of the Neanderthals.]

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The "Flynn Effect": How Applicable is it to Longitudinal IQ Assessment of American University Students?

Ralph Scott & Brian Poncy
University of Northern Iowa

Recent years have witnessed considerable debate concerning the "Flynn Effect" (FE), which takes it name from James R. Flynn, a political philosopher at the University of Otago, New Zealand. Based largely on Flynn's international investigations, the FE rationale assumes that it is a universal phenomenon for aptitude scores to progressively increase over time, FE advocates have translated their findings into recommendations for broad social and educational reform. Thus Flynn (1999) reports that FE investigations endorse "human egalitarian ideals" and (1) challenge the view that "g" is a dominant explanation of human intelligence, (2) support the perspective that environmental factors are far more significant than genetic forces in determining human intelligence, and (3) signal a narrowing of African American - White IQ scores.

Key Words: IQ testing, Flynn effect, WAIS-R, WAIS-3, Raven's matrices

In an indirect endorsement of the Flynn Effect (FE), the American Psychological Association (APA) sponsored a conference on intelligence; proceedings were later published in *The Rising Curve: Long Term Gains in IQ and Related Measures,* edited by Ulrich Neisser (1998). According to Neisser this book constitutes a refutation of *The Bell Curve* (Hernnstein & Murray, 1994) which argued that a close bond exists between IQ, significance of genes to human abilities, upward mobility, and genetic trends. Neisser buttressed his central themes by assuming that increasing IQs can be explained by such environmental interventions as desegregation or forced busing. However, extensive investigations over three decades have failed to document a linkage between forced busing and IQ or achievement benefits (Hanley, 1998; Henry, 1998; Peirce, 1998; Scott, 1983; Williams, 1994). Furthermore, it is to be noted that FE advocates have claimed that Flynn's data indicate a significant

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narrowing of the black/white IQ gap, but in a personal communication with this author Flynn denies that he detected any *significant* narrowing of this gap. Inasmuch as FE advocates urge sweeping schooling reforms based on such dubious interventions as school desegregation, further investigations into presumed IQ increments are warranted.

A central FE premise is that "Without exception whenever the same participants were given both the new test and an older test, they had a higher score on the latter" (Flynn, 1999, p. 5).[1] Commenting on the FE, Kamphaus (1993, p. 120) notes that actual scores on older tests are lower because an examinee must obtain a higher raw score whenever a test is renormed, in order to secure the same standard score previously secured. Despite extensive acceptance within academic circles, the FE prompts unanswered questions. Various researchers have expressed surprise that IQs progressively increase at a time of declining achievement trends (Begley, 1996; Flynn, 1996; Williams & Ceci, 1997). Commonsense suggests that if IQs increase, achievement profiles should follow suit, given high correlations between aptitude and achievement scores. At least seven other issues concerning validity of the FE require clarification.

1. Flynn (1999, p. 8) acknowledges that verbal IQ and achievement test changes over time are "small or nil" in the United States and other English speaking countries. In asmuch as personal and vocational success is closely linked to verbal aptitude and academic competencies, Flynn's statement casts doubt as to the direct meaning of the FE to American educational reform such as proposed by Neisser (1998) and Flynn (1999).

2. The substantial reliance of FE on general fluid intelligence or Gf

[1] This statement is not entirely accurate; exceptions to the FE exist in the literature. As illustrations, see Jensen, 1998, p. 321; Flynn in Neisser, 1998, p. 27. Further and after examining data from a number of FE studies, Lynn (1998, p. 212) concluded that he had been unable to identify a single investigation which appropriately distinguished authentic and spurious IQ increases.

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Table 1

Means, Standard Deviations and Differences:
Female Standard Scores on the WAIS-R and WAIS-3
(N = 22)

	WAIS - R		WAIS - 3		
	Mean	SD	Mean	SD	Difference
Information	10.4	2.3	12.3	2.3	- 1.9
Vocabulary	11.6	2.6	14.0	2.0	- 2.4
Comprehension	13.3	2.3	14.8	1.9	- 1.5
Picture Completion	11.1	1.9	11.6	2.5	- 0.5
Block Design	12.5	2.3	11.9	2.4	+ 0.6
Object Assembly	11.2	2.6	12.1	3.2	- 0.9

	WAIS-R	WAIS-3	Difference
Verbal Scale Prorated IQ	113	123	- 10
Performance Scale Prorated IQ	111	111	0.0
Full Scale Prorated IQ	113	120	- 7

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Table 2

Means, Standard Deviations and Differences:
Male Standard Scores on the WAIS-R and WAIS-3
(N = 19)

	WAIS - R		WAIS - 3		
	Mean	SD	Mean	SD	Difference
Information	11.5	1.7	13.2	2.1	- 1.7
Vocabulary	12.2	1.9	13.8	2.5	- 1.6
Comprehension	12.9	2.8	16.8	3.7	- 3.9
Picture Completion	10.4	3.2	12.3	3.6	- 1.9
Block Design	14.0	2.3	13.6	3.0	+ 0.4
Object Assembly	12.9	3.0	12.0	3.3	+ 0.9

	WAIS-R	WAIS-3	Difference
Verbal Scale Prorated IQ	116	131	- 15
Performance Scale Prorated IQ	117	117	0.0
Full Scale Prorated IQ	119	128	- 9

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Table 3

Means, Standard Deviations and Differences:
All Subjects Standard Scores on the WAIS-R and WAIS-3
(N = 41)

	g loading*			WAIS - R		WAIS - 3		
	WAIS-R African-American	WAIS-R Whites	WAIS-3** All Ss	Mean	SD	Mean	SD	Difference
Information	.79	.78	.81	10.9	2.1	12.7	2.2	– 1.8
Vocabulary	.82	.81	.89	11.9	2.3	13.9	2.2	– 2.0
Comprehension	.81	.76	.80	13.1	2.5	15.7	3.0	– 2.6
Picture Completion	.79	.74	.10	10.8	2.6	11.9	3.1	– 1.1
Block Design	.67	.75	– .02	13.2	2.4	12.7	2.8	+ 0.5
Object Assembly	.66	.66	N/A**	12.0	2.9	12.0	3.2	– 0.0

	WAIS-R	WAIS-3	Difference
Verbal Scale Prorated IQ	114	127	–13
Performance Scale Prorated IQ	114	114	0.0
Full Scale Prorated IQ	115	123	– 8

* *Unrotated first factor generally considered the best measure of g* (Sattler, 1988, p. 148; Kaufman, 1990, p. 254).

** Based in correlations for overall sample and a second principal axis method, employing an oblique rotation (Tulsky & Shu, 1997, pp. 104-105). On the WAIS-3, Object Assembly is an optional subtest and is not included in the factor analysis.

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introduces a limitation in terms of longitudinal learning profiles and practical significance. Flynn (1998, p. 8; Kaufman, 1990, p. 217) attributes two-thirds of FE aptitude gains to the Raven's Matrices (RM) which is heavily influenced by Gf, and significantly less than that to gains in verbal measures which are closely associated with crystallized intelligence (Gc). There is general agreement that Gf is essentially nonverbal, structure free, heavily influenced by adaptability and learning of new information, and highly impacted by genetic factors. But Gf does not easily lend itself to valid longitudinal assessment. Gf increases only until some time during adolescence, after which physiological structures gradually decline (Plomin and DeFries, 1998). Unlike Gf, Gc develops over a longer trend line. Reflecting an individual's genetic endowment but also heavily reliant on verbal abstracting skills, cultural associations, and formal and informal educational factors, Gc cognitive functions do not begin to decline until a person reaches 50 or 60 years of age (Kaufman, 1990 p. 5 1). Therefore Gc cognitive changes are of greater practical significance than Gf IQ directionality.

3. Presumed IQ gains as reported in the FE literature may be associated with the interplay of genetic and biological factors which are profoundly impacted during prenatal and perinatal developmental periods (Lynn, 1998). If nutritional factors have contributed to some authentic IQ increments, the answer to continued IQ gains does not lie in certain popular environmental education reform such as continued school desegregation and "mainstreaming."

4. FE reliance on the Ravens' Matrices (RM) as the "marker" of Gf (Flynn, 1999) and therefore of long-term IQ changes creates other empirical issues. Flynn suggests that the RM may not be all that important with respect to the dynamics of aptitude: "the Raven's Progressive Matrices Test does not measure intelligence but rather a correlate with a weak causal link to intelligence; the same apply to all IQ tests" (1987, p. 187). In fact, the RM presents a number of statistical flaws: one more correct answer on the RM can increase a S's score by one to three points. This is approximately the average gain shown over an entire generation in the Scottish data (Jensen,

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1998, p. 323). Further, various renormings on the RM have not controlled for item standardization.

5. Across FE investigations, experimental variables are "utterly confounded" (Jensen, 1998, p.319). Flynn's core 73 studies involved 7500 Ss from 14 countries, with a CA range of 2 to 48 years. Skills of test administrators are frequently not described and different tests were employed. Further, from 1932 to 1978, when FE investigations were conducted, few FE investigations established equivalence of old and new tests. This introduces a contamination variable: test scores can only be compared over time if item difficulty remains the same (Jensen, 1998, p. 318). Item difficulty is best controlled by administering the same test items, and changing of test items can create spurious increases or declines in the mean raw scores. The WAIS-R is a case in point. In renorming the WAIS-R, steps were taken to simplify directions (Wechsler, 1981, pp. 11 - 19). Words were omitted because they were judged to be too hard or too easy. In item selection, steps were taken to reduce assumed geographic, gender, and ethnic bias. One illustration: a new WAIS-R item asked "Who was Martin Luther King?" (WAIS-R Manual, pp 12-15). Similar changes were made in WAIS-3 renorming (Wechsler, 1997, pp 11-19).

6. Given inconsistencies of experimental variables, one should expect erratic test results; this is precisely what emerges on FE studies. IQ changes fluctuate widely from nation to nation, study to study. Thus in Great Britain the reported increase was two standard points whereas it was 12 in East Germany (Kaufman, 1990, p. 49). Further, Flynn (1987) reported one generation IQ gains which varied from 5 to 25 points in the 14 nations. Separately, Flynn notes that verbal IQ gains extend from virtually nil to 20 points per generation (in Neisser (Ed), 1998, p. 28). It is indeed difficult to ascertain what such diverse results represent.

7. Social class differences produce other obstacles in interpreting the FE phenomenon. In some FE experiments, Ss from lower social classes registered higher cognitive gains than their counterparts representing middle and upper social classes. This reduced the standard deviations and may mask a decline in scores of middle and

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upper social class Ss (Jensen, pp 319-320). Further studies may reveal that the FE reflects a longitudinal decline in scores of higher-achieving persons, or reflect an educational "dumbing down" which promotes learning of less capable students at the expense of the more capable (Turnbull, 1985).

Given the complexity of FE investigations, the present inquiry was designed to determine IQ directionability of a sample of midwestern university students.

Methodology: Aptitude test scores of the same Ss were secured on six subtests of the 1981 and 1997 versions of the Wechsler Adult Intelligence Scale. On both instruments, subtests were selected on their basis of their reported loadings on Gf and Gc. This reflects Kaufman's concern (p.5 1) that Flynn's investigations which employed the popular Wechsler aptitude tests failed to differentiate results obtained on verbal and performance subtests. Kaufman avers that Flynn's use only of Full Scale IQs may have obfuscated differences which existed on verbal and largely Gc tests, compared to results on nonverbal or essentially Gf tasks. Therefore this study employed the Information, Vocabulary, and Comprehension subtests as representing G-c (Sattler, 1988, pp. 4750; Kamphaus, 1993, p. 26). In accordance with these conceptualizations, Block Design, Object Assembly, and Picture Completion subtests constituted Gf measures. Instruments: Both the WAIS-R and WAIS-3 are appropriate for Ss from 16 years through adulthood. The two WAIS versions consist of verbal and nonverbal subtests which comprise the Verbal and Performance Scales, which are combined into Full Scale IQs. Ss employed in norming were stratified on the basis of educational level and controlled for variables of age, geographic region, metropolitan/nonmetropolitan, race, and sex, but not for social class.

Both WAIS versions yield standard scores, with CA-appropriate score means of 100 and standard deviations of 15 for the Verbal, Performance and Full Scales. Statistics concerning subtest scores vary slightly, but generally involve a mean of 10 and a SD of 3. Reliability coefficients on both WAIS versions were corrected for attenuation, and range from the upper 80s to the mid-90s. Correlations between

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the two WAIS versions and other popular aptitude tests are high. Technical information included one comparison of WAIS-R and WAIS-3 scores; congruent with the FE, higher scores appeared on the older version, or WAIS-R. Differences were 1.2 points on the Verbal, 4.8 points on the Performance Scale, and 2.9 points on the Full Scale (Tulsky & Zhu, 1997, pp. 78-79).

Subjects: Ss were undergraduate students at a midwestern university, who volunteered to participate in the study when invited to do so through classroom announcements. Participants were assured of anonymity; each individual was given an identifying number which he/she could cite if they desired an interpretation of their general test performance. Altogether 92 students volunteered to participate. Because of time and resource constraints, only the first 41 volunteers (22 female, 19 male) could be accommodated; they comprise the Ss of the study. The sample consisted of 36 Caucasians, two African American males, one Asian American female, one female Native American, and one male Hispanic. The CA range was 18 to 45 years, with a mean CA of 23.1, and a median of 22 years, and the mode was 22 years (N8). There is no reason to believe that the Ss are not representative of the total number of volunteers.

Testing was conducted by two graduate students, who were trained in administration of the selected WAIS-R and WAIS-3 subtests by a Diplomate of the American Board of Professional Psychology. Following several training sessions, skill quality was assessed prior to initiation of formal testing. Both examiners conducted assessment through cross-balancing. Thus if one S was first given the WAIS-R, the WAIS-3 was subsequently administered; a reverse order of testing (first the WAIS-3, then the WAIS-R was implemented with the succeeding S. For each S, the two test administrations were conducted at intervals of 10 to 14 days.

Results: Table 1, 2, and 3 present subtest means, standard deviations, and differences, as well as pro-rated Verbal, Performance and Full Scale IQs and IQ differences (WAIS-R compared to WAIS-3) for male (N19), female (N22), and All Ss (N41) Ss. Additionally, Table 3 reports, for the first unrotated factor, WAIS-R "g" loadings on the

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six subtests for African Americans and Whites, as well as loadings on five subtests of the WAIS-3 for a general population. The literature shows that nonverbal subtests account for the majority of variance in the second factor ("Perceptual Organization"), which is not reported here since the eigen value on the second variable is quite small. The small eigen value on the second factor attests to the relative significance of Gc and language related aptitude, as contrasted with measures relatively high on Gf, which constitute most of presumed longitudinal IQ gains reported on FE studies.

These tables show that on all three verbal subtests and on one nonverbal subtest (Picture Completion), and contrary to predictions based on the FE, both males and females secured lower scores on the WAIS-R or the older test version. On nonverbal measures and consistent with FE theory, both males and females secured higher scores on the Block Design subtest; male but not female Ss also obtained higher scores on the Object Assembly subtest. The tables also show that for male Ss the Verbal Scale IQs averaged 15 points higher on the older test, whereas it was 10 points higher for females. In a statistical oddity, which the examiner checked and rechecked, no IQ differences, WAIS-R and WAIS-3, appeared on the Performance Scale IQ for either males or females. To some extent this was because on the WAIS-R both males and females obtained higher scores on the nonverbal subtest, Picture Completion.

Discussion: Results of this investigation require qualifications. The N was small and Ss represented only one midwestern university. Only selected subtests were administered, but all verbal tests represent high loadings on the first unrotated factor which is considered heavily weighted in verbal and Gc cognitive functions, as well as academic and vocational success. Also, graduate students conducted the testing; however, this should not have contaminated results, since the graduate students were trained and skill-tested, and scoring procedures were checked by a licensed psychologist.

Certain findings of this study speak to one of Flynn's most intriguing observations: why do achievement scores of Americans decline around a period of rising IQs? A parsimonious answer is to

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simply concur with Flynn that American verbal aptitude and achievement scores may not be rising. Replication studies may affirm Turnbull's evidence that American schools may be downscaling schooling requirements in an effort to adapt to less able students; should this be occurring the ultimate impact of any such cognitive leveling is the subject of another study.

Results of this investigation indicate that additional investigations are needed to assess the relevance of FE theory to longitudinal ability trends of American students. Replications should exert stringent controls on such variables as standardization of Ss, of sample items, and of social class. Before such replications are conducted and results synthesized, it is premature to assume that American educational reforms should be formulated on the basis of the FE rationale.

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Essays in Eugenics

Sir Francis Galton, F.R.S.
With Biographical Introduction by Roger Pearson

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Two Origins of Language Evolution: Unilateral Gestural Language and Bilateral Vocal Language, Hypotheses from IQ Test Data

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This report reviews sex differences in the hemispheric organization of language revealed in standardized data from IQ tests. IQ test data support the notion that language is more bilaterally represented in females than in males. Test data also suggest that females matures faster than males. The factorial pattern of females' IQ, which shows bilateral representation, remains the same throughout life, but the pattern of males' IQ changes from bilateral representation at early childhood to unilateral representation at adolescence and after. Females seeking partners evolved vocal language for emotional and affective communication. Neotenic males were exposed to the vocal language transmitted from nursing females. Vocal language announced from the vocal organ located at the median line was originally bilateral. Gestural language, which is announced from unilateral upper extremities, is another origin of language evolution. Use of hands and arms as language produced "Name Relation" and "Truth Value" through matching between object and hand. The hands are under contra hemispheric unilateral control of the brain. Gestural language is the origin of unilateral hemispheric organization of language.

Key words: sex difference, laterality, IQ, *r*-selection, *K*-selection, gestural language, vocal language, language evolution, sexual precocity, neoteny.

Emotional Language is Neglected in Linguistics

Children during Piaget's stage of preoperational thought, which ranges from 2 to 7 years, develop their abilities to represent objects and events (Wadsworth, 1971, p.70). Language at this stage is studied linguistically. However, language at Piaget's sensory motor

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stage as spoken by fragile children is generally regarded as somewhat primitive or incomplete. "Children prior to this stage (stage of preoperational thought) of development may use words. They may use 'mama' or 'dada' during the first year. These early words are not usually used to *represent* objects, or the early words are not language in the representational sense" (Wadsworth, ibid., p.74). See Figure 1.

Language at the sensory motor stage is possibly an 'emotional language' (Code, 1987, pp.104-106) which "functions as a link between the outside world and our most private inner selves" (Code, ibid., p.106). Adults strengthen emotional language by complex prosody – "the expletive, the soft loving tone, the harsh angry tone, the emotional break in the voice, the wink and the smile" (Code, ibid., p.106-107) – after the acquisition of representational language. Emotional language does not require componential or sequential synthesis or analysis: its impact is immediate and its meaning is fundamental (Code, ibid., p.107). This very human function of language had been neglected as baby talk, but emotional language is closely related to the sexual behavior of females seeking to acquire partners (Hattori, 1998), and could have evolved before representational language.

Sex Differences of IQ and Age Dependence

Lynn (1998) wrote that language processing by females is performed bilaterally, in contrast to male language which is performed unilaterally. The sexual difference found in psychometrics is possibly correlated with the sexual dimorphism revealed by brain science (Breedlove, 1994; Kelly, 1991; de Lacoste, et al., 1986; de Lacoste-Utamsing & Holloway, 1982).

Sexual dimorphism is also age dependent. Allen *et al.* (1991) found that the midsagittal area of the corpus callosum increased with maturation and decreased with advancing age. Blatter *et al.* (1995) reported sexual differences in the shrinkage of cortex with age. Hattori (1999) examined standardization data of IQ tests conducted in Japan, ranging from 3 years to 74 years, and found different patterns in sex difference of IQ. The study of sex differences in IQ should record age (Halpern, 1997).

Hattori's study reviewed three IQ tests with different administration age ranges in relation to human growth (Deacon, 1992; Love &

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Webb, 1996; Wadsworth, 1984, pp.35-171). These were the Wechsler Preschool and Primary Scale of Intelligence, Japanese Edition (Miki, et al., 1961), the Illinois Test of Psycholinguistic Abilities (Kirk, McCarthy, & Kirk, 1968), and the Wechsler Adult Intelligence Scale, Revised Japanese Edition (Sinagawa, et al., 1981). Each test was matched with other editions of the same test developed in another country for cross cultural comparison.

Sex Differences Frequently Eliminated in IQ Tests

There is a problem in studying sex differences in IQ. Sex differences in IQ tests have been intentionally eliminated by test developers. Many psychologists reverse cause and effect, then state that there are no differences in male-female IQs. Sattler (1974) wrote of the Wechsler Intelligence Scale for Children (WISC) and WPPSI that "generally sex differences do not appear to play an important role." Ruschival & Way (1971) concluded that there were no sex differences in IQs. The discussions by many psychologists on this subject are tautological. Matarazzo (1972) states that "From the very beginning of development of the best known individual intelligence scales (Binet, Terman, and Wechsler) took great care to counterbalance or eliminate from their final scale any items or subtests which empirically were found to result in a higher score for one sex over the other." Male-female difference in IQ would not appear on Wechsler type intelligence tests if the tests were developed according to the Wechsler's test development principle to eliminate male-female differences. Sex differences need to be excavated from test data by the sex difference elimination strategy in standardization.

Laterality Observed in IQ Tests:
Bilaterality in WPPSI Prior to Sex Differentiation

The Wechsler Preschool and Primary Scale of Intelligence (WPPSI) (Wechsler, 1967) was developed as a downward extension of the Wechsler Adult Intelligence Scale (WAIS) and is composed of 11 subtests. The age range to which the WPPSI is administered is 3 years 10 months to 7 years 1 month, which coincides with Piaget's stage of preoperational thought. Test data of the WPPSI covering this entire age range were analyzed, since there were no differences between age-sectioned data (Hattori, 1999).

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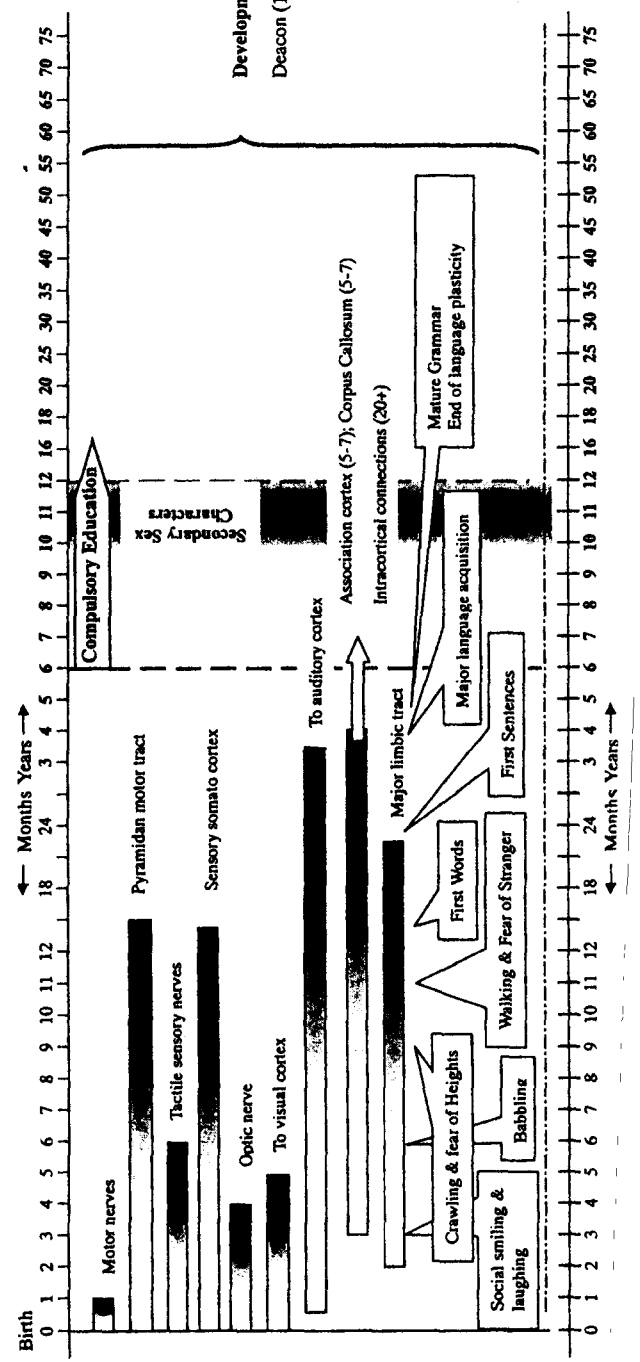
Figure 1 Timings of Brain Development, Piaget's Stages, and Age range of Tests Studied


Birth
0 1 2 3 4 5 6 7 8 9 10 11 12 18 24 3 4 5 6 7 8 9 10 11 12 16 18 20 25 30 35 40 45 50 55 60 65 70 75
← Months Years →
Motor nerves
Pyramidan motor tract
Tactile sensory nerves
Sensory somato cortex
Optic nerve
To visual cortex
To auditory cortex
Association cortex (5-7); Corpus Callosum (5-7)
Intracortical connections (20+)
Major limbic tract
Compulsory Education
Secondary Sex Characters
Development of Brain
Deacon (1992)
Mature Grammar
End of language plasticity
Major language acquisition
First Sentences
First Words
Walking & Fear of Stranger
Crawling & fear of Heights
Babbling
Social smiling & laughing

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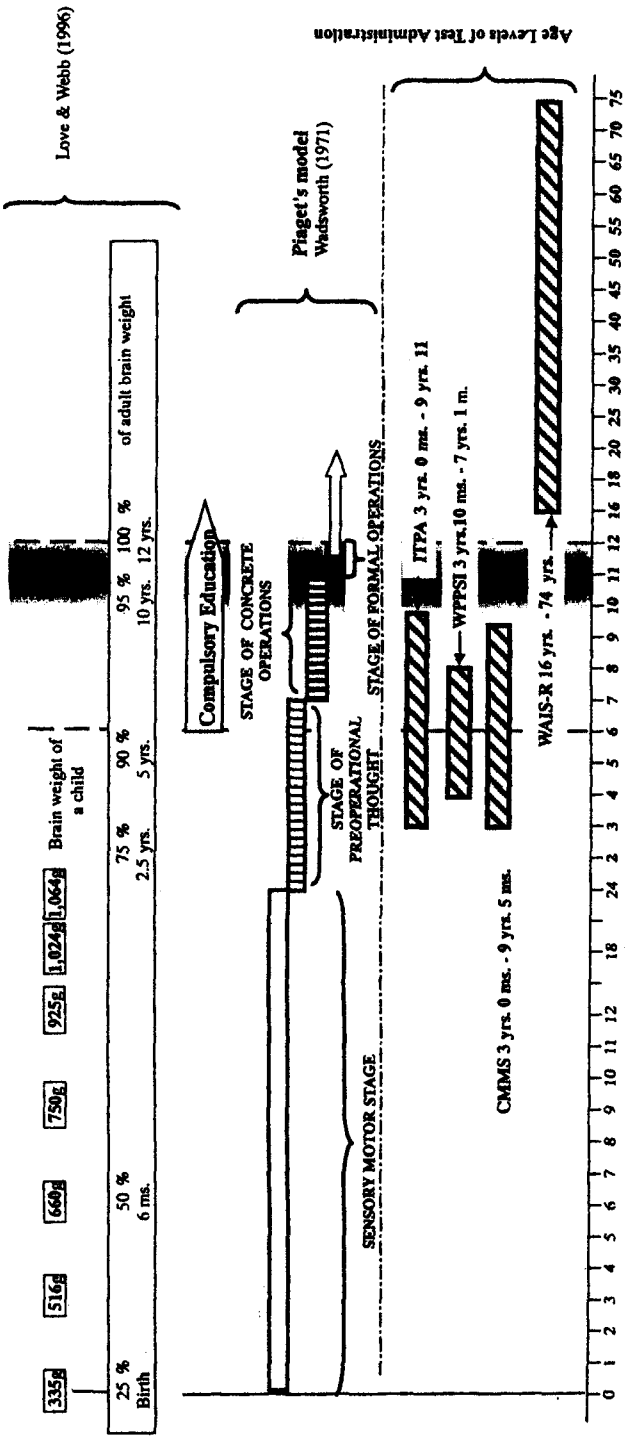
Love & Webb (1996)
Piaget's model
Wadsworth (1971)
Age Levels of Test Administration
335g
516g
660g
750g
925g
1,024g
1,064g
Brain weight of a child
25 % Birth
50 % 6 ms.
75 % 2.5 yrs.
90 % 5 yrs.
95 % 10 yrs.
100 % 12 yrs.
of adult brain weight
Compulsory Education
STAGE OF CONCRETE OPERATIONS
STAGE OF PREOPERATIONAL THOUGHT
STAGE OF FORMAL OPERATIONS
SENSORY MOTOR STAGE
ITPA 3 yrs. 0 ms. - 9 yrs. 11
WPPSI 3 yrs.10 ms. - 7 yrs. 1 m.
CMMS 3 yrs. 0 ms. - 9 yrs. 5 ms.
WAIS-R 16 yrs. - 74 yrs.
0 1 2 3 4 5 6 7 8 9 10 11 12 18 24 2 3 4 5 6 7 8 9 10 11 12 16 18 20 25 30 35 40 45 50 55 60 65 70 75

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Table 1 Sex Differences of Japanese and the US WPPSI (Age Level 3 yrs. 10 ms to 7 yrs. 1 m.)

	Japanese							US					
	Male (N = 300)		Female (N = 291)		Difference			Male (N = 600)		Female (N = 599)		Difference	
	M	SD	M	SD	*d*-Score	*F*	r_{pb}	M	SD	M	SD	*d*-Score	r_{pb}
Verbal Subtests													
Information	9.85	3.09	10.02	3.10	-.06	0.468	-.028	10.15	3.01	9.99	3.06	.05	.025
Vocabulary	10.27	3.00	10.45	2.89	-.06	0.595	-.030	10.09	3.11	9.94	2.95	.05	.024
Arithmetic	10.15	3.09	10.00	2.68	.05	0.395	.023	9.88	3.01	10.14	2.91	-.09	-.042
Similarities	9.93	3.04	9.98	3.04	-.02	0.039	-.008	9.86	3.04	10.16	3.06	-.10	-.048
Comprehension	10.11	2.97	10.03	3.20	.03	0.081	.013	10.01	3.01	9.98	3.03	.01	.005
Sentences	10.45	2.98	10.89	3.60	-.15	2.585	-.076	9.73	2.97	10.23	3.12	-.16	-.079
MANOVA of Verbal Subtests						0.883							
Performance Subtests													
Animal Houses	9.49	3.13	10.55	3.25	-.36	16.276****	-.182**	9.52	2.90	10.46	3.01	-.31	-.155
Picture Completion	10.36	3.12	9.73	2.93	.21	6.579**	.108	10.01	3.06	9.98	2.84	.01	.005
Mazes	10.88	3.35	9.82	3.08	.33	16.083****	.165**	10.38	3.04	9.67	2.97	.23	.115
Geometric Design	10.80	3.15	10.95	3.20	-.05	0.323	-.023	9.68	3.12	10.24	2.97	-.18	-.089
Block Design	10.55	2.87	9.84	3.01	.22	8.501	.113	9.83	3.08	10.19	2.87	-.12	-.058
MONOVA of *Performance* Subtests						10.756****							
MANOVA of all the Subtests						5.576****							
VIQ	100.69	16.01	100.92	15.71	-.01	0.033	-.007	99.86	15.01	100.15	14.94	-.02	-.009
PIQ	102.83	15.45	101.01	15.93	.11	2.002	.056	99.16	15.28	100.72	14.75	-.10	-.050
FSIQ	102.10	16.54	101.15	16.23	.06	0.492	.030	99.52	15.12	100.48	15.00	-.06	-.031

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ **** $p < 0.001$

US data were reprinted from Keiser & Reynolds (1985)

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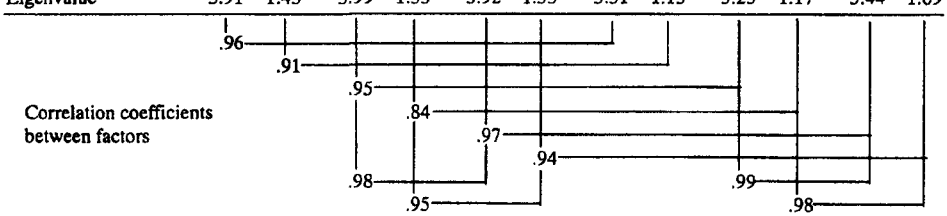
Table 2 Multivariate Factor Analyses (after Varimax Rotation) of Japanese and US editions of WPPSI (Age levels from 3 yrs. 10 ms. to 7 yrs. 1 m.)

	Japanese						US					
	All		Male		Female		All*		Male**		Female	
	591		300		291		1200		600		599	
	Factor		Factor		Factor		Factor		Factor		Factor	
Variable	I	II	I	II	I	II	I	II	I	II	I	II
Information	**.78**	.16	**.78**	.17	**.77**	.14	**.74**	.35	**.72**	.37	**.76**	.33
Vocabulary	**.77**	.03	**.76**	.00	**.78**	.07	**.71**	.29	**.70**	.30	**.72**	.29
Arithmetic	**.49**	**.45**	**.49**	**.45**	**.49**	**.46**	**.58**	**.50**	**.59**	**.48**	**.55**	**.53**
Similarities	**.68**	.18	**.68**	.19	**.68**	.17	**.77**	.15	.77	.17	**.77**	.14
Comprehension	**.72**	.09	**.72**	.09	**.72**	.08	**.78**	.24	**.78**	.20	**.78**	.29
Sentences	**.59**	.28	**.55**	.36	**.60**	.25	**.72**	.24	.72	.19	**.72**	.27
Animal Houses	.30	.37	.35	.33	.21	**.49**	.29	**.60**	.29	**.58**	.28	**.63**
Picture Completion	.37	**.53**	**.41**	**.43**	.36	**.60**	.36	**.60**	.35	**.60**	**.41**	**.56**
Mazes	.10	**.69**	.09	**.67**	.13	**.69**	.14	**.76**	.14	**.79**	.15	**.76**
Geometric Design	.02	**.75**	.05	**.77**	-.03	**.75**	.19	**.77**	.16	**.78**	.20	**.76**
Block Design	.13	**.72**	.17	**.72**	.11	**.70**	.29	**.69**	.30	**.68**	.29	**.70**
Eigenvalue	3.91	1.43	3.99	1.33	3.92	1.53	5.31	1.13	5.23	1.17	5.44	1.09



Japanese Data were calculated from standardization samples.

* Factors of all the US samples were calculated from correlation matrix presented in the WPPSI manual (Wechsler, 1967).

** Factors of male and female of US samples were calculated from correlation matrices presented in Keiser & Reynolds (1985).

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Table 3

Means, Standard Deviations and Differences:
All Subjects Standard Scores on the WAIS-R and WAIS-3
(N = 41)

	g loading*			WAIS - R		WAIS - 3		
	WAIS-R African-American	WAIS-R Whites	WAIS-3** All Ss	Mean	SD	Mean	SD	Difference
Information	.79	.78	.81	10.9	2.1	12.7	2.2	– 1.8
Vocabulary	.82	.81	.89	11.9	2.3	13.9	2.2	– 2.0
Comprehension	.81	.76	.80	13.1	2.5	15.7	3.0	– 2.6
Picture Completion	.79	.74	.10	10.8	2.6	11.9	3.1	– 1.1
Block Design	.67	.75	– .02	13.2	2.4	12.7	2.8	+ 0.5
Object Assembly	.66	.66	N/A**	12.0	2.9	12.0	3.2	– 0.0

	WAIS-R	WAIS-3	Difference
Verbal Scale Prorated IQ	114	127	–13
Performance Scale Prorated IQ	114	114	0.0
Full Scale Prorated IQ	115	123	– 8

* Unrotated first factor generally considered the best measure of g (Sattler, 1988, p. 148; Kaufman, 1990, p. 254).

** Based in correlations for overall sample and a second principal axis method, employing an oblique rotation (Tulsky & Shu, 1997, pp. 104-105). On the WAIS-3, Object Assembly is an optional subtest and is not included in the factor analysis.

tab 3

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There were no significant differences of IQ scores in both the Japanese edition (Miki, et al., 1969) and the U.S. edition (Wechsler, 1967) of the WPPSI (Table 1). Although there were no sex differences in IQs, sex differences appeared in MANOVA's calculated from 6 performance subtests (F=10.76, $p<0.001$) and from all the 11 subtests (F=5.58, $p<0.001$).

Subtests where significant sex differences were found are indicated by shadowed areas of Table 1. Females' higher verbal IQs are seen in both the Japanese and the U.S. editions. The IQs of U.S. females are higher than those of U.S. males. A tendency for females to outperform males in verbal abilities is observed.

Factorial patterns of males and females are quite similar in both the Japanese and the U.S. editions (Table 2). There were no significant differences between correlation matrices of males and females measured by Jennrich tests (1970) of either the Japanese (χ^2 = 56.54, df = 55) or the U.S. (χ^2 = 53.73, df = 55) editions (Hattori, 1999). It is believed that males and females perform WPPSI tasks by similar behavioral strategies.

Factor I is interpreted as verbal factor, and factor II is interpreted as a performance factor coinciding with the test construction of the WPPSI (Wechsler, 1967). However, larger factor loadings of the Arithmetic subtest are found at both factors I and II of the Japanese and the U.S. editions. The *Animal House*, which is a test of the ability to match animal figures and colors, is considered as a downward extension of the Digit Symbol subtest of the WAIS. Therefore, the factorial patterns of the WPPSI are similar to the factorial patterns of WAIS-R females (Table 7). This similarity suggests the course of the sex differentiation process in intelligence. Children have female type intelligence, then develop either male or female type intelligence according the biological sex.

The Beginning of Sex Differentiation and Factorial Patterns in ITPA

The ITPA is a test battery which measures the psycholinguistic abilities of children (Kirk, McCarthy, & Kirk, 1968) from 3 dimensions of communication process: Channels of Communication, Psycholinguistic Process, and Level of Organization according to the Osgood's communication model (1957a; 1957b). The administration

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Table 4 ***d*-Scores, ANOVA's, and point-biserial correlations of ITPA at age level 3 years 0 months to 6 years 11 months**

	Japanese							US						
	Male		Female		Difference			Male		Female		Difference		
Sample size	N=143		N=120					N=238		N=228				
Variable	M	SD	M	SD	*d*-Score	F	r_{pbs}	M	SD	M	SD	*d*-Score	F	r_{pbs}
Auditory Response	36.81	6.19	37.50	5.03	-.12	0.89	-0.06	36.08	5.95	34.66	5.44	.25	5.70**	0.13***
Auditory Reception	39.43	5.59	37.91	6.24	0.26	4.37**	0.13**	36.32	6.39	36.18	6.43	0.02	0.06	0.01
Visual Reception	39.45	6.72	38.93	7.73	0.07	0.33	0.04	36.61	6.17	36.58	6.14	0.00	0.00	0.00
Auditory Association	36.99	6.00	37.45	6.51	-0.07	0.36	-0.04	36.37	6.49	36.13	5.97	0.04	0.18	0.02
Visual Association	38.40	5.81	40.06	6.23	-0.27	4.99**	-0.14**	36.92	6.42	35.56	5.77	0.22	5.79**	0.11***
Verbal Expression	----------							36.26	6.41	36.22	6.06	0.01	0.00	0.01
Manual Expression	37.03	6.06	37.70	6.16	-0.11	0.79	-0.06	36.37	6.15	35.37	6.07	0.16	3.15*	0.08
Grammatic Closure	35.93	5.91	36.46	6.92	-0.08	0.45	-0.04	36.03	6.04	36.29	6.13	-0.04	0.21	-0.02
Visual Closure	----------							35.86	6.81	37.09	5.50	-0.20	4.55**	-0.10***
Auditory Sequential Memory	----------							35.88	6.25	36.29	6.26	-0.07	0.51	-0.04
Visual Sequential Memory	----------							35.32	6.26	36.10	5.73	-0.13	1.97	-0.07*
MANOVA	----------							----------					2.11**	----

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ **** $p < 0.001$

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Table 5 *d*-Scores, ANOVA's, and point-biserial correlations of ITPA at age level 7 years 0 months to 9 years 11 months

	Japanese							US						
	Male		Female		Difference			Male		Female		Difference		
Sample size	N=148		N=107					N=185		N=184				
Variable	M	SD	M	SD	*d*-Score	F	r_{pbs}	M	SD	M	SD	*d*-Score	F	r_{pbs}
Auditory Response	36.81	6.19	37.50	5.03	-.12	0.89	-0.06	36.08	5.95	34.66	5.44	.25	5.70**	0.13***
Visual Response	39.25	7.91	40.90	6.65	-.22	3.07*	-0.11*	36.98	6.19	34.84	5.55	.36	12.24****	0.18***
Auditory Association	34.74	6.12	36.47	4.87	-.30	5.87**	-0.15***	36.80	6.28	35.04	5.82	.29	7.80***	0.15***
Visual Association	36.85	5.90	38.50	5.15	-.29	5.41**	-0.15***	36.15	6.04	35.56	6.02	.10	0.89	0.05
Verbal Expression	----------							36.20	6.09	36.09	6.09	.02	0.03	0.01
Manual Expression	35.09	5.39	36.50	5.02	-.27	4.50**	-0.13**	36.66	5.44	34.72	6.27	.33	10.17***	0.17***
Grammatic Closure	35.16	6.27	38.79	4.63	-.61	25.74***	-0.32***	35.10	6.08	36.33	6.13	-.20	3.74*	-0.10**
Visual Closure	----------							35.58	6.00	35.80	6.32	-.04	0.12	-0.02
Auditory Sequential Memory	----------							35.26	5.87	36.95	6.24	-.28	7.10***	-0.14***
Visual Sequential Memory	----------							35.76	5.95	36.27	6.53	-.08	0.60	-0.04
MANOVA	----------							----------					5.60***	----

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ **** $p < 0.001$

Table 6 Factors (after varimax rotation) of ITPA US Edition and Correlation Coefficients between Factors

Ages 3 yrs. 0 ms. to 6 yrs. 11 ms.	Factor I			Factor II			Factor III			Factor IV		
	All	M	F	All	M	F	All	M	F	All	M	F
Auditory Reception	**.76**	**.79**	**.75**	.00	.03	.11	.03	-.06	.01	-.02	-.05	.00
Visual Reception	**.53**	**.49**	**.51**	.23	.22	-.10	.06	.26	.29	-.34	-.37	-.32
Auditory Association	**.76**	**.75**	**.77**	.12	.10	.07	.15	.20	.17	.20	.13	.25
Visual Association	**.45**	.38	**.51**	.35	**.46**	.22	.09	-.04	.30	-.38	**-.42**	-.24
Verbal Expression	.19	.22	.20	.06	.15	**.76**	**.75**	**.68**	-.10	-.07	-.15	.06
Manual Expression	.06	.07	.04	.02	-.03	**.78**	**.79**	**.79**	.14	.11	.20	.02
Grammatic Closure	**.70**	**.70**	**.72**	.07	.07	.13	.16	.24	.02	.10	.09	.12
Visual Closure	.12	.10	.14	**.70**	**.67**	.33	.30	.33	.71	-.15	-.17	-.09
Auditory Sequential Memory	.15	.17	.12	.08	.07	.06	.08	.07	.10	**.87**	**.86**	**.87**
Visual Sequential Memory	.08	.04	.12	**.82**	**.85**	-.21	-.15	-.08	**.74**	.17	.19	.21
Eigenvalue	2.80	2.84	2.82	1.20	1.32	1.26	1.08	1.07	1.05	1.03	1.00	0.97
Correlations between males & females		.98			.36			.32			.96	

Ages 7 yrs. 0 ms. to 9 yrs. 11 ms.	Factor I			Factor II			Factor III			Factor IV		
	All	M	F	All	M	F	All	M	F	All	M	F
Auditory Reception	**.77**	.05	**.69**	-.13	.16	-.16	-.02	**.83**	-.02	.02	.09	.06
Visual Reception	.36	-.04	.37	-.01	**.77**	.11	**.61**	.28	**.52**	-.24	-.12	-.03
Auditory Association	**.63**	**.52**	**.72**	.28	.37	.23	.21	.30	.05	-.10	.07	-.05
Visual Association	.39	.39	**.42**	.35	.36	**.40**	.37	.13	.35	.25	**.48**	.02
Verbal Expression	.11	**.75**	.04	**.79**	-.17	**.77**	-.16	.09	-.16	.04	-.03	.07
Manual Expression	.04	**.70**	.01	**.74**	.24	**.79**	.26	-.12	.18	-.06	-.06	.00
Grammatic Closure	**.70**	**.53**	**.74**	.19	.17	.09	.09	**.49**	.13	.07	.13	-.04
Visual Closure	.18	.18	.21	.29	**.57**	.35	**.45**	.12	.31	-.15	-.04	-.35
Auditory Sequential Memory	.04	-.11	.05	-.01	-.15	.10	.00	.07	.10	**.92**	**.88**	**.94**
Visual Sequential Memory	-.11	.06	-.09	-.05	**.61**	-.04	**.78**	-.39	**.87**	.26	.35	.09
Eigenvalue	2.58	2.69	2.50	1.14	1.20	1.29	1.11	1.11	1.11	1.04	1.02	0.98
Correlations between males & females		.06			.28			.56			.82	

Correlations between matched Factors of two age groups	.96	.02	.97	-.25	.58	.90	-.44	-.22	.83	.75	.61	.82

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Table 7 Sex Differences of IQ and Subtests in WAIS-R

VIQ and MANOVA of Subtests

	Japanese										US								
	Male			Female			VIQ Differences			MANOVA	Male			Female			Differences		MANOVA
	N	M	SD	N	M	SD	*d*-Score	F	r_{pbs}	F	N	M	SD	N	M	SD	*d*-Score	r_{pbs}	F
16-19	154	101.95	15.85	157	98.03	12.84	.27	5.78**	.14***	1.663	200	99.62	14.76	200	98.49	14.37	.08	.04	------
20-34	163	101.74	14.36	160	99.50	14.18	.16	1.98	.08*	2.564	250	101.33	15.35	250	98.67	14.57	.18	.09**	------
35-54	195	101.34	15.65	159	98.27	15.02	.20	3.11*	.10**	3.864****	250	101.24	15.06	250	98.52	15.21	.18	.09**	------
55-74	233	102.91	16.22	224	96.45	14.30	.41	20.37****	.21****	6.403****	240	101.17	15.07	240	98.98	15.39	.14	.07*	------
All	702	102.09	15.58	700	97.91	14.14	.28	27.58****	.14****	10.219****	940	100.90	15.07	940	98.67	14.68	.15	.07***	------

PIQ and MANOVA of Subtests

	Japanese										US								
	Male			Female			PIQ Differences			MANOVA	Male			Female			Differences		MANOVA
	N	M	SD	N	M	SD	*d*-Score	F	r_{pbs}	F	N	M	SD	N	M	SD	*d*-Score	r_{pbs}	F
16-19	154	100.88	15.67	157	99.28	14.39	.11	0.88	.05	7.065****	200	99.12	14.96	200	99.06	15.30	.00	.00	------
20-34	163	100.8	14.10	160	99.2	15.86	.11	0.92	.05	3.414***	250	101.35	15.34	250	99.14	15.78	.14	.07*	------
35-54	195	98.88	15.79	159	98.48	14.54	.03	0.05	.01	2.139*	250	101.29	15.63	250	98.57	14.73	.18	.09**	------
55-74	233	101.04	15.03	224	98.93	14.93	.14	2.27	.07*	5.966****	240	99.97	14.91	240	99.48	14.71	.03	.02	------
All	702	100.48	15.13	700	98.97	14.91	.10	3.54*	.05*	11.074****	940	100.51	15.19	940	99.06	15.07	.10	.05**	------

FSIQ and MANOVA of Subtests

	Japanese										US								
	Male			Female			FSIQ Differences			MANOVA	Male			Female			Differences		MANOVA
	N	M	SD	N	M	SD	*d*-Score	F	r_{pbs}	F	N	M	SD	N	M	SD	*d*-Score	r_{pbs}	F
16-19	154	101.82	16.23	157	98.31	13.51	.23	4.31**	.12**	4.119****	200	99.39	15.15	200	98.56	14.52	.06	.03	7.74***
20-34	163	101.21	13.97	160	98.95	14.73	.16	2.00	.08*	2.718***	250	101.47	15.57	250	98.71	15.34	.16	.09**	9.17***
35-54	195	100.39	15.93	159	98.33	15.09	.13	1.38	.07	3.049****	250	101.49	15.58	250	98.26	14.86	.13	.11***	10.35***
55-74	233	102.33	15.37	224	97.60	14.62	.31	11.39****	.16****	6.499****	240	101.02	15.06	240	99.25	15.08	.12	.06*	9.12***
All	702	101.54	15.36	700	98.23	14.49	.22	17.22****	.11****	9.571****	940	100.92	15.31	940	98.7	14.92	.15	.07***	------

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ **** $p < 0.001$

Japanese results of subtests and IQ's were calculated from the standardization samples of the WAIS-R Japanese edition.

MANOVA's of US 11 subtests were reprinted from Kaufman *et al.* (1988).

US results of IQ's (r_{pbs}) were calculated from Reynolds *et al.*(1987).

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age range is from 3 years 0 months to 9 years 11 months.

This study used from data the U.S. edition provided by Paraskevopoulos & Kirk (1969, pp.39). Data of the Japanese edition (Ueno, et al., 1993), of which standardization was carried out on a reduced scale and data sets were not complete, were used as reference data. Four subtests, as to which items were not changed in the 1993 revision of the Japanese edition, were not sampled, and only correlational studies were carried out to evaluate the validity of the four subtests.

The ITPA data of the U.S. edition were divided into two age groups of 3 to 6 years and 7 to 9 years in order to evaluate age-sectioned differences. Sex differences were evaluated by MANOVA's on all the 11 subtests. There were significant sex differences in the 3 to 6 age group ($F=2.106, p<0.05$), 7 to 9 age group ($F=5.593, p<0.001$), and the entire age group ($F=5.360, p<0.001$). The smaller F value at the 3 to 6 years old age group as compared with the F at the 7 to 9 years old age group suggests a larger sex difference at the higher age level of 7 to 9. The d-Scores, ANOVA's, and point--biserial correlations are presented on Tables 4 & 5.

Factor analyses were available only from the U.S. edition which has complete sets of data. Factorial patterns were compared by crossing sex and age groups. At the 3 to 6 years-old age group, the correlation coefficients between male Factor I and female Factor I was .98. In the 7 to 9 years-old age group, the correlation coefficient between the male Factor I and female Factor I was .06.

Correlation coefficients are presented on Table 6. Males showed changes of factorial patterns between the 3 to 6 years and 7 to 9 years age groups, but females did not show these changes (bottom row of Table 6). The overall factorial patterns of males and females are similar in the 3 to 6 years age group, but are different in the 7 to 9 years age group.

Male Unilaterality and Female Bilaterality in WAIS-R

The WAIS-R Japanese edition (Shinagawa, et al., 1981) was quite successful in applying the sex difference elimination technique in test standardization. There were no sex differences of IQ measured by ANOVA in the key age group of 20-34 to which other age groups were calibrated. See Table 7.

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Table 8 Sex Differences of WAIS-R of Pooled Entire Age of 16 to 74

	Japanese									US								
	Total 1402		Male 702		Female 700		Difference			Total 1880		Male 940		Female 940		Difference		
	M	SD	M	SD	M	SD	*d*-Score	F	r_{pbs}	M	SD	M	SD	M	SD	*d*-Score	F	r_{pbs}
Verbal scales																		
Information	10.01	3.13	10.43	3.16	9.58	3.04	.27	26.315****	.137****	9.40	2.99	9.82	3.11	8.98	2.81	.28	----	.14****
Digit Span	9.77	2.85	9.9	2.91	9.64	2.79	.09	2.828*	.046*	9.41	2.89	9.37	2.93	9.44	2.85	-.02	----	-.01
Vocabulary	9.82	3.04	10.36	3.14	9.29	2.85	.35	44.296****	.179****	9.34	3.02	9.41	3.06	9.25	2.97	.05	----	.03
Arithmetic	9.93	2.92	10.31	3.10	9.54	2.68	.26	25.316****	.133****	9.38	2.98	9.87	3.03	8.89	2.84	.33	----	.17****
Comprehension	10.06	3.00	10.39	3.04	9.73	2.92	.22	17.189****	.111****	9.47	3.08	9.60	3.10	9.33	3.06	.09	----	.04**
Similarities	10.19	3.03	10.32	3.06	10.06	2.99	.09	2.602	.043	8.99	3.21	9.01	3.22	8.98	3.19	.01	----	.00
Performance scales																		
Picture Completion	10.20	2.89	10.48	2.81	9.93	2.95	.19	12.844****	.096****	8.91	3.13	9.15	3.00	8.66	3.25	.16	----	.08****
Picture Arrangement	10.01	3.06	10.03	3.10	9.99	3.02	.01	0.073	.007	8.76	3.11	8.97	3.10	8.54	3.10	.14	----	.07***
Object Assembly	9.83	3.04	10.13	3.01	9.53	3.05	.20	13.936****	.099****	8.79	3.01	9.16	3.10	8.42	2.87	.25	----	.12****
Block Design	10.14	2.90	10.28	2.92	10.00	2.87	.10	3.363*	.048*	8.80	2.99	8.95	3.04	8.65	2.93	.10	----	.05**
Digit Symbol	10.04	2.97	9.81	3.13	10.26	2.79	-.15	7.950***	-.076***	8.54	3.36	8.08	3.23	8.99	3.42	-.27	----	-.14****
VIQ	100.00	15.02	102.09	15.58	97.91	14.14	.28	27.581****	.140****	99.79	14.92	100.90	15.07	98.67	14.68	.15	----	.07***
PIQ	99.72	15.03	100.48	15.13	98.97	14.91	.10	3.543*	.050*	99.79	15.15	100.51	15.19	99.06	15.07	.10	----	.05**
FIQ	99.89	15.02	101.54	15.36	98.23	14.49	.22	17.219****	.111****	99.81	15.16	100.92	15.31	98.70	14.92	.15	----	.07***

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ **** $p < 0.001$

Japanese results of subtests and IQ's were calculated from the standardization samples of the WAIS-R Japanese edition.

US results of subtests (r_{pbs}) were calculated from Kaufman *et al.* (1988)

US results of IQ's (r_{pbs}) were calculated from Reynolds *et al.*(1987).

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Table 9 Factor structures of sex differentiated WAIS-R data of the pooled age level: 16-74

	Male				Female			
	Japanese		US		Japanese		US	
Factor	I	II	I	II	I	II	I	II
Information	.83	.21	.81	.28	.83	.23	.83	.27
Digit Span	.51	.28	.46	.39	.22	.56	.48	.40
Vocabulary	.88	.23	.90	.25	.87	.24	.89	.26
Arithmetic	.70	.36	.63	.39	.53	.54	.60	.46
Comprehension	.74	.17	.75	.27	.74	.18	.76	.33
Similarities	.75	.28	.62	.49	.77	.33	.70	.44
Picture Completion	.32	.65	.34	.67	.34	.62	.41	.66
Picture Arrangement	.33	.62	.33	.63	.35	.54	.40	.57
Object Assembly	.26	.77	.28	.80	.25	.72	.29	.79
Block Design	.06	.81	.21	.76	.15	.76	.21	.69
Digit Symbol	.37	.46	.27	.65	.11	.66	.24	.65
Eigenvalue	5.21	1.20	6.12	1.28	5.12	1.20	6.27	1.19

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Although sex differences in IQ were negligible at the age levels of 20-34, MANOVA's using 11 subtests of the WAIS-R Japanese edition revealed that there were significant sex differences at all four age levels: 16-19, 20-34, 35-54, and 55-74. This means that the sex difference eliminating test development technique of WAIS-R worked exactly as intended, but that sex differences could not be suppressed by such artificial test development strategy.

Subtests of the pooled overall age range of the Japanese and the U.S. editions were examined. Both the Japanese and the U.S. males outperformed females in logico-verbal subtests: Information, Arithmetic, Comprehension, and in performance subtests: Picture Completion, Object Assembly, and Block Design. Both the Japanese and the U.S. females outperformed males at Digit Symbol, which is classified as one of the verbal subtests and requires memorizing patterns and matching different kinds of signs.

Factorial patterns of sex differentiated data of Japanese and U.S. WAIS-R editions were quite similar in spite of socio-cultural differences between the two countries. The factorial patterns of males in both the Japanese and the U.S. editions showed possible unilateral hemispheric processing of language and those of females in both editions showed bilateral hemispheric processing of language.

This is interpreted as indicating that factor I of males is the verbal factor and factor II is the performance factor. These two factors are considered to be independent of each other. Each variable has a larger factor loading on either of these two factors, but not on both of the factors. Tasks in the performance subtests are considered to be processed unilaterally by the right hemisphere of the brain. Unlike males, factor loadings of Digit Symbol and Arithmetic subtests of females are larger in both factors I and II. The factorial pattern of females shows possible bilateral processing of language in two hemispheres of the brain.

Sex Differentiation in IQ and Growth of Intelligence

"The mass of evidence seems to support the notion that there are significant differences in hemispheric organization of language between males and females. Results of unilateral brain damage studies as well as studies with normal subjects support the view that

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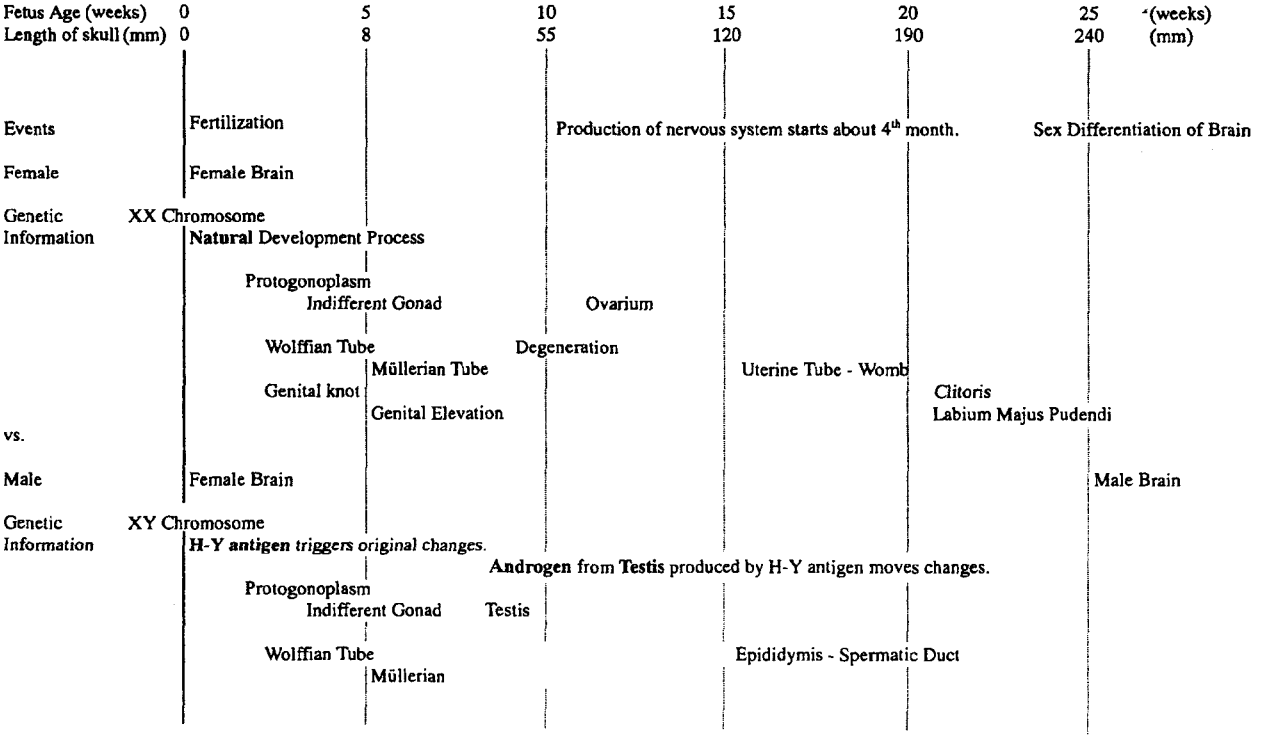
Sex Differentiation of Genital System



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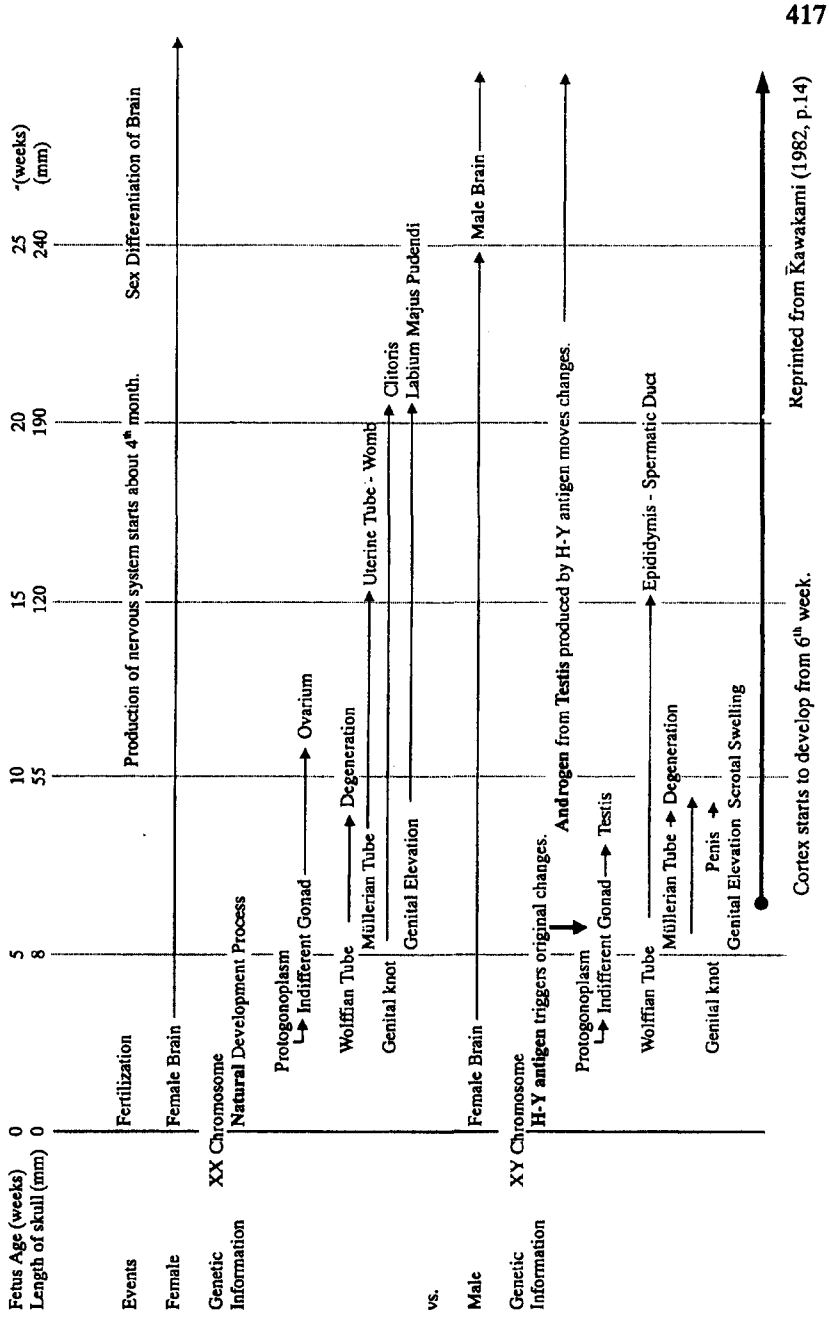

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language is more bilaterally represented in females than in males" (Code, 1987, p.13). This characteristic of sex differences is revealed in a review of IQ tests administered at different ages: WPPSI, ITPA, and WAIS-R. There is no sex difference at the younger age level, possibly until 6 years old, but the sex difference begins to appear possibly after 6 years of age, and the sex difference is obvious at the adult level. Factorial patterns of females remain the same, probably throughout life. Unlike the females, factorial patterns of males change with growth from a pattern similar to that of females' (which shows a bilateral hemispheric processing of language) to the characteristic male pattern (which shows a unilateral hemispheric processing of the language). It seems as though the male's pattern branches off from the female's factorial pattern as it reaches maturity.

The Male Brain and the Female Brain:
Hormone and Sex Differentiation in the Brain

All the human brain has female sex at the point of initial production of neural system (Kawakami, 1982, pp.14-20). In contrast to the female brain, which follows a natural development process, the male brain follows a rather complicated process. The XY Chromosome of the male cell determines the discharge of H-Y antigen, which triggers a change of undifferented gonads into testis. Then androgen from testis produced by H-Y antigen activates a change of the female brain into a male brain. This differentiation is non-reversible, and is estimated to happen at the $5^{th.}$ to $7^{th.}$ month after fertilization.

Hormones play a significant role in the sex differentiation process. "The brain as a set of living nerve cells floating within a sea of hormonal environment, responds sensitively to a change of hormonal environment, and excitability nerve cells and modes of synaptic connection always change" (Kawakami, 1982, p.120). The sex differentiation is not limited to brain regions related to genital systems such as hypothalamus, lobus before pituitary gland, and pituitary gland. Dörner and Staudt (1968) reported morphological sex differentiation at the nerve cell nidus of the hypothalamus. Gorski, et. al. (1980) reported that there was a sex differentiation in the volume of nerve nidus of field in front of the inside optic tract of rats. Toran-Allerand (1976) reported that testosterone and 17β-estradiol accelerated growth in the axon and formation of nerve

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circuit net by tissue culture experiment of a field in front of optic tract and hypothalamus anterior part. Arai & Matsumoto (1978) reported that growth of nerve circuit net of arcuate nuclei was accelerated by 17β- estradiol. The effects of sex hormones on the thickness of the right and left hemispheres are estimated from adrenogenital syndrome (Ehrhardt & Baker, 1973).

Sex Differentiation Programmed at the Fetus Period

"Boys and girls mature at different rates" (Lynn, 1994). "The adolescent growth spurt occurs in all children.... The peak velocity of growth in height averages about 10 centimeters a year in boys, and slightly less than this in girls. In boys, the spurt takes place on average between 12.5 and 15.5 years of age, and in girls some 2 years earlier. Girls are more advanced in maturity at all ages from birth onwards. The sex difference in height in adulthood is due mostly to the longer period of male growth" (Tanner, 1996, p.101).

However, it is interesting to discuss the parameters of physical growth such as height in relation to brain volume. The brain does not grow at the same pace as physical growth. At birth, a neonate's brain weight is about 25% of the adult's brain weight. The neonate's brain weight increases to about 50% of adult's brain weight at 6^{th} month after birth. It increases to about 60% of the adult's brain weight at 12^{th} month. Brain weight increases up to 75% at the 2^{nd} year after birth. About 10 years old, brain weight reaches 95% of the adult level. After puberty, by about 12 years of age, brain weight reaches adult level (Love & Webb, 1992). Spurt theory (Epstein & Toepfer, 1978) had been denied by Mann (1984), who surveyed a large number of data by other researchers including Epstein & Toepfer.

It is appropriate to think that sex differences are already programmed during the fetus period. The mechanisms of sex differentiation of the neural system at early childhood and those of sex differentiation of physical structure and genital organs at puberty are different. Although a process at which hormone influences the formation of the brain area is not known at the present level of brain research, it is quite plausible that hormone secreted during the fetus period influences the formation of the brain areas. "Sex hormone acts on neural cells, then promotes developments of axon and

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Table 10 Sex Differences of CMMS Japanese Edition

	Male			Female			Difference		
	N	M	SD	N	M	SD	*d*-Score	F	r_{pbs}
3 yrs. 0 ms. to 5 ms.	31	29.35	7.44	29	26.97	7.12	.32	1.609	.165
3 yrs. 6 ms. to 11 ms.	29	29.48	7.78	31	31.81	5.19	**-.35**	1.875	-.178
4 yrs. 0 ms. to 5 ms.	30	29.30	9.88	30	31.67	6.50	**-.28**	1.201	-.143
4 yrs. 6 ms. to 11 ms.	29	33.31	8.62	31	33.23	6.64	.01	0.002	.006
5 yrs. 0 ms. to 5 ms.	30	36.17	8.85	30	37.47	7.21	**-.16**	0.389	-.081
5 yrs. 6 ms. to 11 ms.	30	39.30	7.34	30	40.00	7.19	**-.10**	0.139	-.049
6 yrs. 0 ms. to 5 ms.	30	37.03	6.07	30	37.47	7.05	**-.07**	0.065	-.033
6 yrs. 6 ms. to 11 ms.	29	34.21	7.50	31	38.29	5.85	**-.59**	5.567**	-.306**
7 yrs. 0 ms. to 5 ms.	31	33.97	8.82	29	33.59	6.99	.05	0.034	.024
7 yrs. 6 ms. to 11 ms.	30	33.80	7.60	30	38.00	6.95	**-.56**	4.991**	-.290
8 yrs. 0 ms. to 5 ms.	29	34.52	7.06	31	34.77	7.72	**-.03**	0.018	-.017
8 yrs. 6 ms. to 11 ms.	30	34.57	7.83	30	37.37	6.85	**-.38**	2.173	-.192
9 yrs. 0 ms. to 5 ms.	31	37.52	8.09	29	37.76	5.24	**-.04**	0.019	-.018

** $p < 0.05$

dendrite, also promotes formation of synapse" (Kawakami, 1982, pp.116117). "Neurocyte divides several times and loses its function to synthesize DNA, then moves from ventricular layer to intermediate layer" (Tsumoto, 1986, p.29). Sex differentiation of intelligence as observed in behavior at puberty does not occur as a result of a mechanism which activates at the puberty; rather sex differentiation at puberty has been programmed to differentiate at puberty as a result of a genetic information which is possibly programmed at the fetus level.

There should be a mechanism in the neural system to direct the secretion of the sex hormones at puberty; otherwise, the sex hormones would be secreted arbitrarily. It is more logical to hypothesize that the neural system develops according to a genetic program, and that once the sex differentiation of the neural system reaches puberty the neural system sends signals to secrete the sex hormones again. Spurt theory uses an opposite concept.

Females Mature Faster than Males

Faster maturity of females compared with males is found in the data of Columbia Mental Maturity Scale (CMMS), the Japanese edition (Misawa, et al., 1982). Female superiority, or the faster maturation of the female, is found at 10 age levels out of all the 13 age levels of the CMMS. See the shadowed rows of the Table 10.

In the WPPSI Japanese edition, trends of female superiority are found in seven subtests: Information; Vocabulary; Similarities; Sentences; Animal Houses; and Geometric Design out of 11 subtests, and at VIQ. In the WPPSI U.S. edition, the evidence of female superiority are also found at Arithmetic, Similarities, Sentences, Animal Houses, Geometric Design, Block Design, VIQ, PIQ, and FSIQ. In spite of test development strategies which eliminate sex differences (Matarazzo, 1972), female superiority appeared on the WPPSI administered at ages 3 to 6.

In the ITPA Japanese edition, evidence of female superiority is found in Auditory Association, Visual Association, Manual Expression, and Grammatical Closure of age level 3 to 6. Evidence of female superiority are also found in Auditory Response, Visual Response, Auditory Association, Visual Association, Manual Expression, and Grammatic Closure at age levels 7 to 9.

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In the ITPA U.S. edition, evidence of female superiority is found in Grammatic Closure, Visual Closure, Auditory Sequential Memory, and Visual Sequential Memory at age level 3 to 6. Similar evidence is observed at Grammatic Closure, Visual Closure, Auditory Sequential Memory, Visual Sequential Memory at the age level 7 to 9.

Although the two age groups were sampled independently according to stratified sampling programs, sex differences are observed in the same subtests of the two age groups. Female superiority in a specific age group is regarded as faster maturation of the female.

Different Selection Pressures on Males and Females

Different Selection Pressures

Sex differences were found in the IQ tests WPPSI, ITPA, & WAIS-R. If ontogeny repeats phylogeny, the ontogenic sex differences could be hypothesized by phylogeny. There could be different selection pressures between males and females. The phylogeny of *Homo* should be able to explain the faster maturation of females compared with males.

Female's Faster Growth, '*r* selection', and Precocity

Sex differences in brain weight are observed 6 months after birth (Mann, 1984). The mechanism of sex differences in brain or intelligence should be explained by a mechanism which does not presuppose adolescent growth spurt. The faster growth in females is explained by an evolutionary pressure of '*r* selection,' as proposed by MacArthur & Wilson (1967). When the number of individuals was smaller than the carrying capacity of the environment, it was advantageous for a fertile female to produce as many children as possible. "For mammals, the intrinsic rate of population increase depends on gestation period, litter size, interbirth interval, age at sexual maturity, and lifespan" (Martin, 1992, p.96). In the case of *Homo*, the environment in which *r* selection could operate was provided by 'colonization,' as suggested by Gould (1977, p.326). Enough food for survival was obtained for small groups of *Homo* in new land acquired by colonization.

Homo was not able to shorten the gestation period. Even at the evolutionary stage of fossil man, the neonate could be an 'ectopic

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fetus' (Portman, 1941). The further shortening of gestation could have produced a high risk neonate which could die without medical care. The neonate *Homo* would have a large skull – the container of the brain. The brain sizes of Kebara Neanderthals were 1200 to 1750 *ml.* and the range of modern human races are about the same (Stringer, 1992, p.247). The estimated brain size of *Homo erectus* varies between 750 to 1250 *ml.* The womb of *Homo* was not large enough to increase litter size. The birth canal is at its maximum size for bipedaling and delivery. The *Homo* neonate, which was delivered as an ectopic fetus, had to be taken care of for quite a long period of time after delivery. There could be a limit to shorten the interbirth interval; otherwise the fragile neonate *Homo* would be thrown into the dangerous external environment without adequate support. The only possible strategies for *Homo* were sexual precocity and a longer lifespan. The female *Homo* matures faster than the male and the life span of the female is longer than that of the male.

Homo gradually migrated from its original place of evolution to new lands where the population density of *Homo* was low, and possibly migrated again to another land before the colonized land became crowded. *Homo* lived in a depopulated environment where *r* selection was the best evolutionary strategy. The *r* selection demanded that the female *Homo* should grow rapidly (Gould, 1977, p.351). Female *Homo*'s sexual precocity, which is observable from the appearance of sex symbols, was acquired.

r selection presupposes a quite low population density. "Measures of old world population size on the eve of the transition to agriculture, some 12,000 to 10,000 years ago, come from estimates of the maximum population density that this way of life could sustain. These generally range from 5 to 10 million people" (Landers, 1996, p.404). It could be difficult to find a partner in such a depopulated environment. Unlike promiscuous chimpanzees, the bipedal female *Homo* did not have the means to signal her mating season. The female had to evolve some means to transmit the signal in a thinly populated environment.

Precocity should have been accompanied by faster growth of language. "Humans spread into seasonal climates such as subtropical Africa and Asia (between 1.5 and 1 million years ago) and subsequently into temperate Eurasia (around 500,000 to 400,000 years

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ago)..... Interestingly, this later geographic expansion was associated with an increase by a third in the size of the human brain" (Trinkaus, 1992, pp.348-349). *Homo* evolved language and intelligence during this era of geographic expansion.

Male's Slower Growth, '*K* selection', and Neoteny

The relatively slower growth of males is explained by an evolutionary pressure of '*K* selection' (MacArthur & Wilson, 1967). "Population size stabilizes at carrying capacity (*K*), when breeding is approximately balanced by food availability and predation. In practice, of course, a population that has reached carrying capacity fluctuates for various reasons, but its level can remain roughly constant over time as long as the environment stays stable" (Martin, 1992, p.96). Under *K* selection, it is advantageous for an individual to reproduce a smaller number of individuals who are better adapted to the environment. Since *Homo* gradually spread through Eurasia by exploiting virgin lands one by one, enough food could be obtained by migration of the excess population. *Homo* could have migrated into new lands while looking for food provisions. Predation rather than food availability drove the *K* selection of the *Homo*.

Under the *K* selection, where intraspecific competition is accelerated, neoteny is an appropriate strategy to acquire characteristics best adapted to the environment. In order to be a winner in intraspecific competition, advantageous characteristics within the species must be maintained as much as possible and disadvantageous characteristics should be abandoned. Intelligence and masculinity, the qualities necessary for hunting, were developed.

Ontogenic development of the organs of an individual is principally carried out by a differentiation schedule determined phylogenically. However, velocity of the development is accelerated during phylogeny, and condensation occurs. "Rates of development must speed up during phylogeny so that descendants pass through the ancestral part of ontogeny faster than the ancestors did; this condensation may occur by deletion of stages or by accelerated development" (Gould, 1977, p.480). The condensation disturbs phylogenically determined schedules of differentiation of organs. During the course of evolution, a certain organ of a descendent begins to differentiate at an earlier timing that is different from the original timing

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possessed by the ancestors. By this deviation of the differentiation timing, known as heterochrony, characteristics which could have appeared at adult stage of the ancestors begin to appear at a younger stage in the descendent. If the timing of differentiation is early enough, in other words, there is time for further growth. Hypermorphosis, which is "the phyletic extension of ontogeny beyond its ancestral termination" (Gould, 1977, p.482), occurs in the organ. Acquired advantageous characteristics are accelerated beyond the level of the ancestors, and the characteristics of the descendent reach more advantageous levels than the level reached by the ancestors. The neoteny under *K* selection is an effective strategy to acquire advantageous characteristics. Gould (1977, p.344) wrote:

> Ancestral rates of differentiation are maintained and delayed maturation permits their extrapolation beyond ancestral conditions. But maturation and differentiation may be strongly tied in development; delay of one leads to retardation of the other if the morphology of that delay is favored in a *K* regime. If both are delayed at the same rate, we obtain a proportioned giant and no heterochrony in the strict sense. But slow development often leads to the retention of juvenile growth rates and proportions. A general 'matrix of retardation' presents juvenile features to selection at ancestral adult sizes. If advantageous, they can be retained and delayed even further to yield neoteny.

Neoteny is also an effective strategy to abandon disadvantageous characteristics. The neotenic individual does not grow to an ontogenic stage at which genes carrying disadvantageous characteristics are transcribed. "Loss of an adult stage [either by truncation or retardation] usually leads to the loss of genes transcribed only late in the ontogeny of ancestors" (Gould, 1977, p.340).

Neoteny becomes more effective when acquiring characteristics than aid in selecting a partner and in transmitting the advantageous characteristics to the descendants. Neotenic retardation produces further hypermorphosis and strengthens competitive characteristics. Competitive males, who could provide more food and were more attractive to females, had better chances to mate with females. Retardation in sexual maturation acquired by neoteny was an advantageous strategy. "I suspect that the primary determinant of

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hypermorphosis is retardation of sexual maturation rather than somatic differentiation" (Gould, 1977, p.343).

Hattori (1998) hypothesized that the evolution of language and intelligence was promoted by the female's needs to send signals to a male when she was ready to accept a selected partner. For males, it is important to enhance their ability to interpret signals transmitted from females. Due to the extended duration of maturation as a result of neoteny, a longer duration to learn female's language was available to the males. "The newborn human child is about as dependent a creature as we find among placental mammalian infants. This dependency is then extraordinarily prolonged, and the child requires intense parental care for many years. The flexibility of childhood persists during more than a decade of necessarily close contact with adults. The adaptive premium thus placed on learning (as opposed to innate response) is unmatched among organisms" (Gould, 1977, pp.400-401). "Delay in development enabled him to develop a larger and more complex brain, and the prolongation of childhood under conditions of parental care and instruction consequent upon memory-stored and speech communicated experience, allowed him to benefit from a more efficient apprenticeship for his conditions of life" (de Beer, 1959). Male *Homo* had more chances to develop communication skills by slower growth.

Two Origins of Language Evolution

The Evolution of Emotional Language

Code (1987, p.13) wrote about significant sex differences in the hemispheric organization of language, but was cautious enough to introduce opposing reports which indicated problems in experimental design before reaching a conclusion (Code, ibid., pp.13-14). However, the standardized data of WAIS-R Japanese and U.S. editions revealed that sex differences were consistently observed in these two environments: Japan and the U.S. The results of the factor analyses of the WAIS-R in Japan and the U.S. suggested that "there are significant differences in hemispheric organization of language between males and females" (Code, ibid., p.13).

"An important function of language is to communicate affective or emotional states and the right hemisphere has been heavily implicated in both the control of emotion and the processing of

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affective/emotional language" (Code, 1987, p.88). If "language is more bilaterally represented in females than in males" (Code, ibid., p.13), it is plausible to think females' language is related to 'emotional language' (Code, ibid., p.99). Hattori (1998) hypothesized that language evolved in females, who used language as a medium to transmit favorable signals to their preferred males. Corballis (1999) wrote, "The alarm calls of monkeys and the hooting of apes are in some form related to human spoken language." Emotions are attached to both fear and affection. Unlike other hominoids, *Homo* introduced vocalization to express affection. The first human language was a love song rather than a howl of warning (Money, 1997). Such language should be closely related to affective emotions. However, this hypothesis does not explain any evolutionary pressure to generate representational language which possesses 'Name Relation' (Carnap, 1947, pp.96-98) and 'Truth Value' (Carnap, ibid, pp.26-27).

Laterality of the Brain

Controversy raged over "whether it is the case that language is lateralized to the left hemisphere at birth, or whether the new born baby starts with a hemispheric bilateral potential for language which gradually lateralized to the left with development" (Code, 1987, p.14). Two opponent discussions were introduced from cases of unilateral brain damage, commissurotomy, hemispherectomy, or hemidecortication (Code, ibid., pp.8-12). Dennis & Whitaker (1976) concluded, "Hemispheric equipotentiality does appear to make an untenable supposition about the brain because it neither explains nor predicts at least two facts about language – that the two perinatal hemispheres are not equally at risk for language delay or disorder and that they are not equivalent substrates for language acquisition." However, such discussions on laterality should be interpreted with caution.

Myelination of corpus callosum is estimated to occur from 5 to 7 years of age (Deacon, 1992). It is difficult to apply a pathological model which was developed out of adult pathology using an adult language model, to 3 to 5 year-old children whose normal growth stages of the brain do not possess the full functions of the adult brain. Many of the discussions presupposed a representational or semantical

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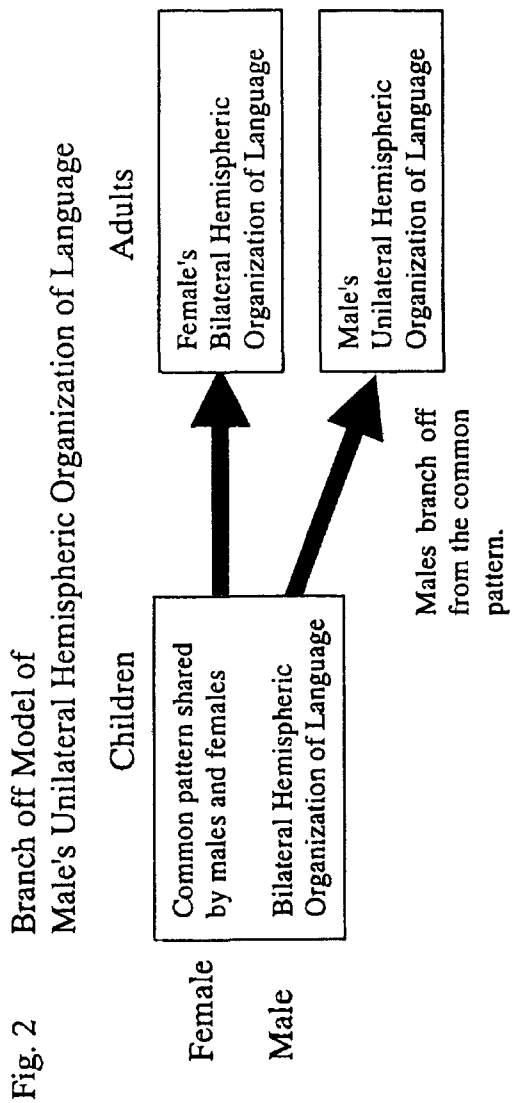
Branch off Model of
Male's Unilateral Hemispheric Organization of Language
Children
Adults
Female
Male
Common pattern shared by males and females
Bilateral Hemispheric Organization of Language
Female's Bilateral Hemispheric Organization of Language
Male's Unilateral Hemisphere Organization of Language
Males branch off from the common pattern.

Fig. 2

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aspect of language which is typically observed at the mature adult level language. Aphasiology had not yet developed tools to explicate pre-representational or pragmatic aspects of the language. The aphasiology on children provides us precious information, but the discussions on laterality should be started from normal population but not from clinical data.

It is not easy to measure intelligence at earlier stages of child development when the intelligence and sensory motor skills are not differentiated. Language has not yet developed to a stage measurable by test batteries. There is controversy over hemispheric equipotentiality, and a conclusion has not been reached yet. However, it is quite evident that there are clinical facts from which two opposing conclusions on hemispheric equipotentiality: "language is lateralized to the left hemisphere at birth", or "the new born baby starts with a hemispheric bilateral potential for language which gradually lateralizes to the left with development" (Code, 1987, p.14) are deduced.

The opposing discussions about the hemispheric equipotentiality of children sound quite odd when the bilateral character of female brains is generally accepted. The differentiation process of the human brain provides us a key to this controversy. The brain at its first stage of the differentiation process is a female brain (Fig. 2). The male brain is produced by hormones acting on this female brain. If ontogeny repeats phylogeny, there should be an ontogenic stage of brain development at which both male and female individuals have the same laterality pattern as the adult female. It is estimated from females, whose language is bilaterally processed, that both male and female children process language bilaterally, and that males begin to differentiate in the direction of unilateral processing while females continue to grow toward the direction of bilateral processing.

Evolution of Language with Name-Relation

Hemmer (1969) regressed facial length with cube root of cranial capacity, then concluded that the difference between the basic *erectus* group and present day *Homo*. The latter which have relatively large containers for the brain should have more neotenic characteristics than the basic *erectus* group. *Homo*'s larger brain volume was accompanied by a loss of developed jaw musculature and formation of strong crests (Gould, 1977, p.398). *Homo* became less able to eat

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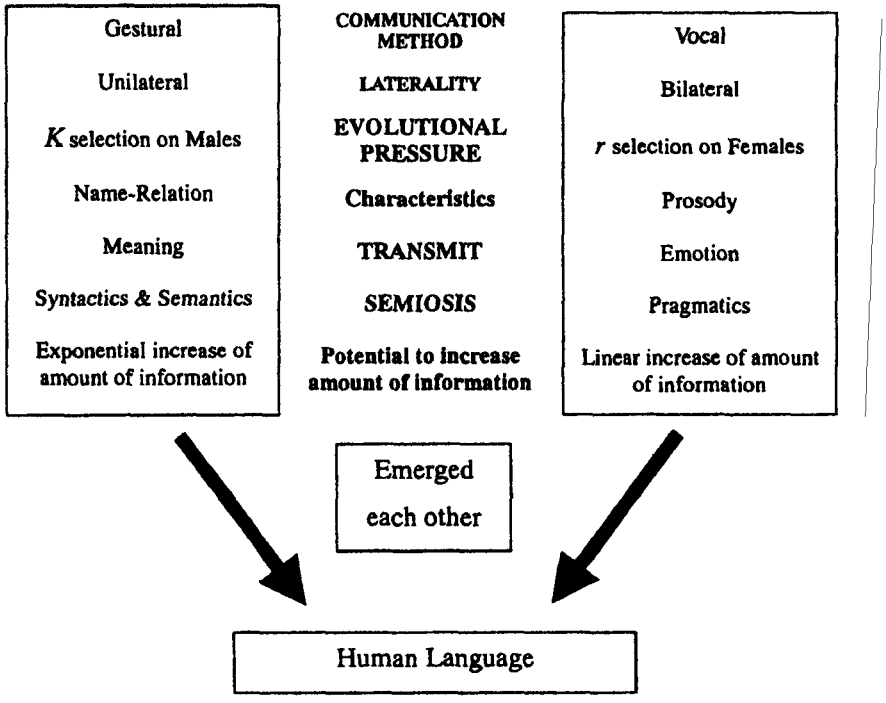
Fig. 3 Model of Language Evolution



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foods without processing before consumption, and had to develop skills to process foods. The most primitive chopping tool was appropriate for scavengers who had begun to increase brain weight and to lose jaw musculature and formation of strong crests.

The upper extremities that were released from transportation activities were ready to evolve further. The use of right and left hands was differentiated to improve food processing skills. In order to process a food, the food to be processed must be held by one hand and the force to process the food must be added by the other hand. There should be a correspondence between two differentiated hands: a hand holding a food and another hand holding a tool. The correspondence was later extended to the food itself and to the tool itself. By the processing of foods, *Homo* acquired relation of 'correspondence'. The acquisition of correspondence is not enough to establish 'Name Relation'. "Truth Value as the extension of a sentence" (Carnap, 1947, pp.26-27) must be established. *Homo* had to hit a food held by one hand by a tool held by another hand. As a result of 'right' action, the *Homo* was able to acquire processed edible food. *Homo* acquired 'Name Relation' and 'Truth Value'.

"Each hemisphere is concerned primarily with sensory and motor processes on the contralateral side of body" (Kandel, 1991, p.8). The spoken language is announced from the vocal organ which is located at the median line of the body, but the language is controlled by language areas located at the left hemisphere. Corballis (1999) gave us a suggestion to solve this paradox by saying, "Human language may have evolved from manual gestures" (Corballis, ibid.). Gestural communication explains the evolution of the left hemisphere of the brain. If the evolution of the language had started its evolution from verbal communication carried out by vocal organs located at the median line, unilateral localization of the left hemisphere would not have occurred. Corballis' explanation of language as originated from manual gestures is quite plausible. Nishimura et al. (1999) reported, "activation of the auditory area in the sign-language task." Nishitani & Hari (under review) studied cortical activation to manipulation tasks by MEG and found that Brodmann's area 44 and 45 known as Broca's area responded to the manipulation tasks. The use of the right hand for communication could have involved speech centers on the left hemisphere of the brain. Corballis (ibid.), who discussed that

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150,000 to 10,000 years are too short to evolve a complex grammatical structure, was quite correct. The gestural language, which is produced out of asymmetric structure: right hand and left hand, had a potential to increase its capability to carry 'amount of information' (Bar-Hillel, 1964) exponentially by a combination of two sets of information. If language were evolved from the verbal language which came out from symmetric vocal organs, language would not substantially increase the amount of information within a short period of evolution.

Emergence of Gestural Language and Vocal Language

Unilateral hemispheric characteristics of the gestural language had an advantage in effective processing of signals, since interhemispheric communication is not necessary; and the unilateral hemispheric signal processing was further reinforced by the use of the hand and tools for food processing. The use of the hand for processing objects evolved gestural language which has Name Relation. Meantime, emotional language using voice appeared. Emotional language evolved a phonetic system which has prosody. Gestural language, which has 'Name Relation' and 'Truth Value', and the emotional language, which has prosody, evolved along with each other.

Human language has two evolutionary origins. From the sequence of ontogeny, it is estimated that the emotional language first appeared then the representative language followed. Parrots can imitate phonetic sequence but cannot understand meaning. The sex differentiation is observed in the ontogenic sequence from children to adult male and adult female. Sex differences of IQ are explainable by the life-history strategy of males and females.

Humans, who were dispersed in broad open land or in woody areas evolved mixed communication modes by recruiting advantages of gestural communication and vocal communication. The gestural communication, which had advantages of complexity due to hemispheric use of brain, recruited vocal communication, which had the advantage in transmission of emotion, or vis-a-vis. The gestural communication could have evolved into a stage capable of designating and communicating complex information, but it had a disadvantage as a medium of remote communication. Precise movement of fingers

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was not visible from a long distance. Visibility is very poor within dense forests. *Homo* evolved language which had capabilities of transmitting emotion, handling complex 'Name Relation', and remote communication.

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Social Organization, Group Conflict and the Demise of Neanderthals

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The article suggests that violent conflict, neglected by recent scholarship, was a key factor in bringing about the Neanderthals' demise in the Middle to Upper Paleolithic transition. Relying on the general archaeological evidence regarding group sizes and drawing an analogy from historical hunter-gatherer societies, the author argues that Cro-Magnon man probably had large, 'tribal', groupings as opposed to the Neanderthal's small ones. *Inter alia* this would have given *Homo sapiens sapiens* a decisive advantage in warfare.

Key Words: Neanderthals, Cro-Magnons, Upper Paleolithic, Social Organization, Group Conflict.

The Neanderthals and their fate have been the subject of intense controversy for more than a century. Despite vastly expanding knowledge, the mystery still seems far from resolved, and the argument shows no signs of ebbing. There is now a growing consensus that, at least in Western Europe, the Neanderthals were replaced by *Homo sapiens sapiens* Cro-Magnons and did not contribute substantially to the modern gene pool. Otherwise, most of the old questions remain, inevitably converging on the fact of the replacement itself. Why did the Neanderthals lose out in the competition which took place in their own specialized habitat, Europe of the Ice Age, to which they were physiologically and behaviorally adapted? This apparent outcome necessarily overshadows the discussion, for it surely tells us something about the Neanderthals and about the ways in which they differed from modern man. Theories explaining the Neanderthals' demise abound. Was their Mousterian tool-kit less efficient than that of Upper Palaeolithic *Homo sapiens sapiens,* for example in hunting (and fishing)? Was their family structure looser, providing less male support to the infants? Did he lack *Homo s.s.'* planning depth (Binford 1985, 1989; Soffer 1994), or advanced lingual capabilities (Lieberman 1989, 1993; Davidson and

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Noble 1989; Chazan 1995)? Where not these aspects of an inferior symbolic capability, evident in the artistic 'creative explosion' which marks the Middle to Upper Palaeolithic transition?

All these explanations may be correct to some degree, and they are probably mutually reinforcing rather than mutually exclusive. However, the aim of this article is to point out an additional, and potentially crucial, factor, which curiously has been almost entirely overlooked. I wish to suggest, first, that, like all known modern hunter-gatherers, Upper Palaeolithic *Homo sapiens sapiens* had regional groupings, whereas the Neanderthal had not. Some scholars have argued more or less along similar lines, emphasizing the advantages of these extended networks for economic cooperation among the newcomers (Wobst 1976; Gamble 1976, 1983, 1986; 1996; Stringer and Gamble 1993; Pfeiffer 1982; Gilman 1984; White 1985; Parker and Milbrath 1993; Soffer 1994; Shreeve 1995; Mellars 1996). This article, however, claims that regional grouping also conferred a decisive, inescapable, advantage in conflict.

The premise of this article, shared by the majority of experts on the subject, is that Upper Palaeolithic, anatomically fully modern, *Homo sapiens sapiens* was 'biosocially' similar to living *Homo* (e.g. Trinkaus, 1989a, 1989b; Knecht, Pike-Tay, and White 1993); that is, fully possessed of the biological endowments that would make possible all of the subsequent socio-cultural development of *Homo*. The overwhelming notion that those who created the amazing symbolic representations of the Upper Palaeolithic are practically 'us' is widely held. If this is agreed, then a carefully-controlled analogy from the full range of historically known hunter-gatherer societies is one of our most useful tools for gauging Upper Palaeolithic social life (taking into account the points made e.g. by Clark 1968; Gamble 1986; Foley 1988; Parker and Milbrath 1993; Steele and Shennan 1996; Lourandos 1997). As we know, *all* modern hunter-gatherer societies exhibit a regional group structure ('dialect tribe'; Birdsell 1968), averaging 500. In these the local, extended family groups of around a few tens come together, usually with an intermediate 'clan' grouping of about 150-175 people. This structure is universal, with its 'magical numbers' evident in all habitats, climates, and forms of social stratification. In more arid and well-watered environments the local groups would normally be widely dispersed, coming together mostly

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during annual meetings, in which common ritual and ceremonies are performed and marriages are agreed upon. In more lush environments, the local groups and larger clans of the regional group might live closer together. The regional group is a unique social body, integrated by the overlapping ties of kinship, culture, and social cooperation.

Archaeology seems to support our premise regarding the analogous social behavior of Upper Palaeolithic *Homo sapiens sapiens.* Not only do his sites indicate larger and denser population then before; his vast trade networks are evidence of regional connections, while his elaborate art is widely interpreted as a means of social communication in larger societies (Mellars 1973, 1996; Wobst 1976; Gamble 1976, 1983, 1986; 1996; Stringer and Gamble 1993; White 1982, but 1985, 1993; Pfeiffer 1982; Gilman 1984; Soffer 1994; Shreeve 1995). How does the Neanderthal compare with this? As scholars increasingly stress, behavior and social life is probably the most neglected aspect of Neanderthal research (e.g. Tattersall 1995: 158; Mellars 1996; Steele and Shennan 1996). Still, there seems to be an agreement that Neanderthal groups were very small and barely in touch with one another. The only line of investigation to suggest otherwise (Aiello and Dunbar 1993; Steele 1996) is also the only one I am aware of that specifically ventures estimates of group sizes. However, the Aiello-Dunbar study is largely based on mathematical speculation. Attempting to infer from neocortex size to group size and language development, Aiello and Dunbar suggest that Neanderthal group size was similar to those of Cro-Magnon man (and both in fact were slightly greater than ours); for all these humans the predicted group size was around 150. Given the simple correlation that underpins the study, this similarity is unsurprising, for, as is well known, average Neanderthal brain size was slightly larger than ours. However, similar neocortex size alone tells us nothing about the apparent *dissimilarities* between the Neanderthal and modern man. Indeed, whereas it is difficult to check the Aiello and Dunbar estimates with respect to *Homo erectus* or the Neanderthal, it is easily checked with respect to the extant species in the two extremes of their spectrum. And in both cases it does not do well. While the chimpanzee predicted group size of 60 is not far from the mean observed one (53), that for the Gorilla is 67 as opposed to

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the observed 7, and for Pongo (orangutan) it is 60 as opposed to the observed 2 (Steele 1996). Furthermore, as mentioned earlier, modern hunter-gatherer average group size is around 500 rather than the figure claimed by Aiello and Dunbar.

By contrast, archaeological evidence would seem to support the view that Upper Palaeolithic *Homo sapiens sapiens,* like historical hunter-gatherers, had regional groups (presumably, like the modern, averaging 500), whereas the Neanderthal lived in small groups, perhaps of about a few tens like the chimpanzees or *Homo sapiens* local group, and had at best much looser and narrower regional ties. A computer simulation suggests that the minimum stable mating universe averages 150-200 individuals (Wobst 1974), which might mean that Neanderthal females, like those of the chimpanzee and some other animal species, simply left their groups at puberty and joined other groups. In the case of the Neanderthals, this may or may not have involved closer inter-group ties between the local, extended family groups than is the case among the chimpanzee. This seems impossible to tell. Either way, full-fledged regional groups, in the form with which we are familiar today, seems to have emerged with *Homo sapiens sapiens.*

If this is so, what are the consequences? As mentioned earlier, some scholars have pointed out the clear economic advantages of wider social networks. They have argued that these trade *cum* information *cum* culture *cum* marriage 'alliances' (as they have called them) would have given *Homo sapiens sapiens* a considerable competitive advantage over the Neanderthals, for they would have served to diffuse information about hunting and other resource and risk factors, fostered cooperation in communal big game hunting, and provided some measure of insurance, in the form of mutual aid, against occasional stresses and shortages. In my view, this theory is persuasive. However, it glaringly leaves out one of the major functions of regional groups among hunter-gatherers, that is, cooperation in conflict and fighting. Only peaceful functions of the 'alliances' are mentioned.

There are deep historical reasons for this omission. It is a reaction against older notions epitomized by H. G. Wells's depiction of 'The Grisly Folk' exterminated by the superior *Homo sapiens sapiens.* During the Rousseauite 1960s, peaceful relations were

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assumed to be the norm among aboriginal groups, allegedly only spoiled by the advent of agriculture and of 'civilization'. The life of the Kalahari Bushmen hunter-gatherers, then under study, was interpreted in this vein, and generalized to all hunter-gatherers. This also seemed to tally with the influential theories of Noble laureate and co-founder of ethology, Konrad Lorenz (1966), according to which animals in nature did not kill co-specifics; the implication was that humans had somehow gone astray in their later evolution. Finally during the controversy over the 'out of Africa' versus the 'multiregional' view of the origin of modern humans, proponents of the former school, such as Stringer (Stringer and Gamble 1993: 193-7), recoiled from the charge that they espoused the 'politically incorrect' ideas of 'racial superiority' and 'global genocide'. In any case, interpretations in terms of relative competitive advantage, either somatic or reproductive (Zubrow 1989), had a much more modern sound to them than any based on violence.

As it happens, most of these 1960s' views have since been all but reversed. In the first place, later research has found a great deal of intraspecific killing taking place in many species, including our cousins, the chimpanzees (useful summaries in Carpenter 1974; Hausfater and Hrdy 1984; Huntingford and Turner 1987; Van Hooff 1990; Dennen and Falger 1990; also Tiger and Fox 1971: 209-10; and for the chimpanzees: Bygott 1972; Teleki 1973; Goodall 1986;'Itani 1982; de Waal 1996; Wrangham and Peterson 1997). There is no reason to substitute the assumption that aboriginal humans were the exception in *not* killing their co-specifics for its earlier held opposite. Simultaneously, it has been increasingly realized that the Kalahari Bushmen were a marginalized group, pressurized by its agricultural neighbors into an extremely and environment, and hardly representative of the full range of Pleistocene hunter-gatherers (Wilmsen and Denbow 1990; Shott 1992). Furthermore, it has become clear that they, too, experienced a very high deadly violence rate, and that fighting among them stretched far back to pre-Bantu times (Lee 1979: 398; 1982: 44; Woodhouse 1987; Campbell 1986). The same applies to the mid-arctic Eskimo, another 'harmless people' in 1960s anthropology, which were exceptionally dispersed in one of the harshest environments on earth. As Briggs (1994: 156) has revealingly written: 'Readers of Canadian Inuit ethnography, my own

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Never in Anger (1970) in particular, have sometimes concluded that Inuit are always and everywhere pacific. Nothing could be farther from the truth'. The evidence from the whole range of hunter-gatherer societies shows extensive violence and warfare taking place among them. One comparative study of 99 hunter-gatherer bands belonging to 377 different cultures found that practically all of them engaged in warfare at the time of the study, or had ceased to do so in the recent past (Divale 1972). According to another study (Ember 1978), in ninety percent of hunter-gatherer societies there was violent conflict, and most of them engaged in inter-group warfare at least every two years, like or more than the rest of human societies. The author of yet other comprehensive cross-cultural studies similarly concluded that 'the greater the dependence upon hunting, the greater the frequency of warfare.' (Otterbein 1991; also Keeley 1996) By far the best model is Australia, our largest, continent-size, 'pure' laboratory of simple hunter-gatherers, which before Western arrival was wholly unaffected by contact with either farmers or herders. The focus on the Kalahari Bushmen has resulted in a relative neglect of this methodologically and empirically far superior 'laboratory' in recent anthropological literature. The continent was the home of an estimated 300,000 hunter-gatherers, distributed among 400 to 700 regional groups which averaged 500-600 persons each (see Lourandos 1997: 36-8 for considerably larger estimates). The Aborigines' hunter-gatherer way of life was of the simplest sort there is. Still, warfare was widespread among them in both and well-watered environments, sparse and relatively dense population densities (Wheeler 1910: 118-9, 141-59; Warner 1930-1, 1937; Meggitt 1962:38, 42; Pilling 1968: 158; Strehlow 1970: 124-5; Tacon and Chippindale 1994). The same applies to other vast 'reservations' of historical hunter-gatherers: the Indians of the American Northwest (Krause 1970 [1885]: 169-72; Boas 1966: 108-10; Drucker 1951: 337-41; 1965: 75-81) and of the Great Plains (even before the latter's adoption of the horse and gun; e.g. Ewers 1975; Schlesier 1994); and the Eskimo of Alaska and Greenland (Nelson 1983 [1899]: 327; Oswalt 1967: 185-8; Burch and Correll 1972; Burch 1974). All exhibited regional grouping, territoriality, and endemic hostilities.

What we learn from historical hunter-gatherers is the common sense proposition that relations among regional groups spanned the

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full range from cooperation to competition and conflict. The proponents of Upper Palaeolithic regional networks and 'alliance' overlooked conflict altogether. Other scholars did sometime mention that violence may also have been involved in Neanderthal-Cro-Magnon relations. As Stinger has written: 'many different kinds of interactions could have occurred (and probably did occur), ranging from avoidance to tolerance to interbreeding, and from conflict and economic competition to friendship and exchange of ideas.' (Stinger and Gamble 1993: 193; also Tattersall 1995: 202-3) Nevertheless, Stringer, for example, went on to minimize the violent aspect, espousing the view that 'the fate of the Neanderthals... was one of a gradual displacement to more marginal and less favorable environments rather than defeat in some sort of territorial battle.' (Stinger and Gamble 1993: 197) This view raises some fundamental questions, and perhaps the first of which is the following: if the Neanderthals were not pushed out by force to these marginal and less favorable environments, why did they leave the better ones?

The proponents of a fundamentally peaceful replacement would argue that the Neanderthals simply retreated from, or went extinct in, the better environments because they were out-competed, for whatever reason, for example in hunting. But a typical reaction of out-competed humans, especially in unregulated, 'anarchic' societies, is to resort to violence. Conflict is substituted for competition. Even if we assume that the Cro-Magnon men did not resort to violence, why didn't the Neanderthals? After all, a great deal was at stake. Humans (and other species) jealously guard their hunting territories against intruders. Drawing an analogy from the European colonial expansion, Diamond (1992: 44-48) has reminded us that the Amerindians, for instance, did not just sit still when the white man killed off the bison upon which their livelihood depended. However, it was precisely to the colonial analogy that the proponents of a fundamentally peaceful replacement have objected; for one (of many) crucial differences was that the Cro-Magnons seem to have possessed nothing like the overwhelming military superiority enjoyed by the Europeans in the age of colonialism, 'with one side packing all the resources, fire-power, and know-how to triumph in an unequal contest.' (Stringer and Gamble 1993: 194; following Graves 1991)

Indeed, if the process in the Middle to Upper Palaeolithic

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transition was not predominantly peaceful, were not the Neanderthals formidable rivals? They were more strongly built than *Homo sapiens sapiens,* quite intelligent, and experienced close-quarters hunters. It is a matter for debate whether the differences in stone technology would have significantly mattered in an armed conflict. *Homo sapiens sapiens* may have used elementary projectile weapons but had not yet invented the bow. Diamond has suggested that like the isolated populations of the Americas and Australia at the time of Western expansion, the Neanderthals may have lacked natural immunity to epidemics brought by the invaders. It is uncertain, however, whether the Cro-Magnons had anything like the resistance developed by the sixteenth century's dwellers of the open and largely urban Eurasian landmass. Diamond has also suggested that *Homo sapiens sapiens'* superior lingual communication helped to decide the issue in his favor. Better communication may surely have been an advantage. But communication may serve as a clue to a far greater potential advantage with which communication is inextricably related. For, as is widely acknowledged, better communication was probably one of the principal prerequisites of larger social groupings. And if it is indeed the case that *Homo sapiens sapiens* maintained regional ('tribal') group ties, whereas the Neanderthals had not or had much weaker ones, this would have been an overwhelming power advantage, of precisely the type that the proponents of a predominantly peaceful replacement believe to have been absent. Cooperation among regional group members would have created a decisive numerical superiority over the much smaller and isolated Neanderthal local groups.

The interrelationship between language, symbolic culture, communal ritual and ceremonies, and advanced sociability, widely associated with Upper Palaeolithic *Homo sapiens sapiens,* is thus explained from an additional direction. Communal art, culture, and ritual, whose adaptive rationale would otherwise appear quite mysterious, may prove to have had a crucial adaptive value. The proponents of large peaceful networks in the Upper Palaeolithic have interpreted them, in a modern spirit, as information systems. While this interpretation is plausible, it seems to me to be narrow and one-dimensional. As we know only too well, communal art, culture, and ritual, are at least as much a divisive as they are a communicative

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medium between groups. Indeed, as an older generation of functionalist scholars held, to what they seem to contribute most is to inner cohesion within groups (e.g. Durkheim 1965). 'Amphictionic alliances', as they are called after the ancient Greek example, have been widely observed among historical hunter-gatherers (and agriculturists) to have tied together regional groupings of ritual and war. Anatomically fully-modern *Homo sapiens sapiens'* successful kit for inheriting the earth may thus have included the following interrelated and mutually-reinforcing elements: more sophisticated tools; a more advanced communication system; a better planning and modelling capacity; and a crucially larger social structure in the form of the regional group, fostered by art, ceremony, and ritual. This regional grouping was highly significant for social cooperation between remote kin; and almost inevitably it would seem to have been *Homo sapiens sapiens'* trump card in intergroup conflict.

The principle involved is far from being new. The notion that larger and stronger grouping was of prominent significance in human evolution dominated by inter-group fighting was suggested by Spencer and Darwin ([1871]: Ch. v, 496-500), and was commonly held by late nineteenth and early twentieth centuries evolutionists. More recently it has been advanced by Alexander and Tinkle (1968), and Alexander (1971, 1987: 79, 1989). It was central to Bigelow (1969); and was also raised by Hamilton (1975: 146), Baer and McEachron (1982), and Shaw and Wong (1989). However, while advancing the principle, none of these authors has attempted to apply it to any specific evolutionary context or prehistorical chronology, and most of them were not careful to differentiate between biological and cultural evolution (but see Vine 1987: 67-8; also Aiello and Dunbar 1993: 191, in my view incorrectly). It is clear that subsequently, once larger societies, like chiefdoms, states and empires, developed by way of cultural evolution, they possessed an overwhelming advantage over smaller aggregates of power. By contrast, my view is that regional grouping is not to be regarded as one of *Homo sapiens sapiens'* many cultural inventions. Instead, like the very potential to create culture, or complex language, that is, advanced symbolic networks, the capacity for regional group relations, with which advanced symbolic capability is intimately linked, seems to have been a skill which fully modern Upper Palaeolithic *Homo sapiens sapiens* had evolved biologically.

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Like language, regional grouping, as a minimum, exists wherever fully modern *Homo sapiens sapiens* live. This may give a new meaning to Aristotle's definition of the members of our species as political animals by nature.

Does this mean that we are back to the extermination scenario of earlier generations to explain the extinction of the Neanderthals? Undoubtedly, to a large degree we are, but perhaps in a more refined form. No out-and-out massacre is suggested, though some massacres probably took place. If the Neanderthals were increasingly pushed out into more marginal and harsher environments, where they progressively went extinct, this was probably done by force. However, the use of violence was in all likelihood much more frequently covert than overt. Again we may learn this from the pattern of hostilities among historical aboriginal humans (and within other species). Hostile demonstration to keep rivals and competitors away or convince them to retreat is by far more common than the deadly encounter itself. Deterrence, which is of course underpinned by the implied and actual use of force, is much more widespread than actual deadly violence. Applying this pattern to the Middle to Upper Palaeolithic transition, the far larger size of *Homo sapiens sapiens'* groups may have been in itself sufficient to persuade the Neanderthals to withdraw when showdowns over hunting and foraging territories would occur, as they inevitably did with the newcomers' arrival. The larger group's superior force prevailed in a clash when demonstration and deterrence failed, as they occasionally must have. As the Neanderthals were forced into worse and worse environments, economic competition was increasingly tilted against them. But this was a competition tilted not only by *Homo sapiens sapiens'* other presumed advantage but also by his superiority in conflict. Needless to say, other forms of interaction between the Neanderthals and Cro-Magnons may also have taken place across the highly diverse geographical and time span of the Middle to Upper Palaeolithic transition, including peaceful and even co-operative interactions. This admixture is in the nature of inter-group relations, and is evident in any successful human colonization.

The predominance of hostile demonstration and deterrence in the conflictual situation (backed by the actual use of force) may contribute to explaining the scarcity of archaeological evidence for

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violent confrontations between the Neanderthals and Cro-Magnons (Trinkaus and Shipman 1993: 416). Furthermore, the problem of archaeological evidence for intraspecific violence is in fact much broader and applies to all of prehistory. The evidence from the Pleistocene is notoriously patchy and inconclusive, not least that which might indicate warfare. Stone axes and spearheads are dual-purpose and could have been used only for hunting. Wooden shields, leather body amour, and tusk helmets – familiar from historical hunter-gatherers – normally do not preserve well. In view of the scarcity of sedentary settlements, evidence of ruin in raiding (similarly familiar from modern hunter-gatherers) would be exceedingly ambiguous. Hunting- and daily-life accidents are difficult to distinguish in fossilized injured bones from those caused by fighting, although comparative studies do suggest the widespread existence of the latter (Vencl 1984; Roper 1969; Keeley 1996). Moreover, the evidence of hostility, territoriality, and fighting as an integral aspect of historical hunter-gatherer inter-group relations is the best behavioral model we can bring to bear on our Palaeolithic ancestors.

Obviously, what has been suggested here is relevant not only to *Homo sapiens sapiens'* colonization of Europe but to his remarkable spread all over the Old World. Whether or not *Homo sapiens sapiens* completely replaced all other humans, as seems to have been the case in Western Europe, or partly interbred with them in some parts of the world, it is clear that we are dealing here with an outstanding global success, achieved remarkably swiftly in Palaeolithic terms. We now know that Neanderthals and (perhaps not fully modern) *Homo sapiens sapiens* may have lived together in the Middle East for tens of thousands of years. In Europe the Neanderthals apparently survived in the Iberian *cul de sac* for somewhat longer than was earlier realized. Still, once the process got going, it was unprecedentedly sweeping. After all, it was not conducted by purposeful armies and navies, but in all likelihood by the gradual expansion of hunter-gatherer communities following resources along the line of least resistance. Greater skills of all sorts probably helped *Homo sapiens sapiens* to out-compete more archaic humans, but if they reacted violently what would have been his recourse? Greater cerebral capacity does not necessarily translate into success in

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conflict, especially when mounted on a more slender physique, as was the case with *Homo sapiens sapiens.* Large group cooperation in conflict may have proved the decisive factor in dealing with other humans who had no still-fledged regional groups at least to the same degree that large group cooperation in hunting proved economically efficient.

To be sure, this does not mean that *Homo sapiens sapiens'* directed conflict behavior solely towards the Neanderthals or other archaic humans. *Homo sapiens sapiens'* regional groups were probably no more united in bonds of brotherhood among them than the European powers would be in the age of Western colonialism. Cooperation, competition, and conflict among them undoubtedly took place, as they did among historical hunter-gatherers. However, as with all 'arms races' – natural or human-generated – when all sides possessed regional group ties, as was the case with *Homo sapiens sapiens,* a 'punctuated equilibrium' of sorts prevailed among them. More archaic humans, who possessed none, or much weaker ones, were outnumbered, deterred, overwhelmed, progressively pushed to the margins, and out-competed.

The argument advanced here is largely theoretical. It is based on an analogy from historical *Homo sapiens sapiens* regional groups – in peace and in conflict – as well as on the evidence about the extent of the social networks of the Neanderthals and Upper Palaeolithic *Homo sapiens sapiens.* In the author's view, the analogy and evidence are compelling. It is hoped that by offering a fresh perspective on a key unresolved problem, this paper can stimulate new archaeological research into the question of conflict in the demise of the Neanderthals, as well as, perhaps, a new examination of existing finds which at present seem to be too scant to support an answer, one way or the other.

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The Pheromone Androstenol: Evolutionary Considerations

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Cowley & Brooksbank have shown that female exposure to androstenol increases the number and depth of female interactions with males. However, in nature, the only females customarily close enough to males to be affected by male-emitted androstenol would be sleeping and cuddling partners, i.e. mates or potential mates. Thus, the observed behavioral response is probably a mechanism for promoting bonding with mates and the evaluation of potential mates. Recognizing an evolutionary purpose for the female reaction to androstenol makes one feel more confident that the observed effects are real.

Key Words: Pheromones, androstenol, bonding, mate selection

Human females exposed to 5a-16-androsten-3a-ol (androstenol) overnight interact more with males than unexposed females do. The experimental procedure (Cowley & Brooksbank, 1991) involved having students wear overnight a necklace designed to release androstenol. Next morning the subjects filled in a questionnaire regarding who they had interacted with (i.e. who they had spoken with, including greetings), who initiated the interaction, and the depth of the interaction ("i.e. the extent to which he/she felt the exchange encompassed personal involvement."). No effect was found for males, or for female interaction with females, but the females reported a significant increase in interactions with males. For instance, the control females reported 3.08 interactions that morning with males, while those females exposed to androstenol overnight reported 5.42 interactions with males, a difference that had probability of occurring by chance of .024. The measure of depth rose from 34.2 to 97.4,

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significant at .011. The measure of duration rose from 6.09 to 26.7, significant at .001.

The authors of the article offer very little explanation for what function the rather strong effects of androstenol serve, saying at the end merely that "it appears that exposure to androstenol induces 'approach' response in females." Earlier (p. 657), the authors had stated "clearly from a sociobiological point of view the benefit would be to the male signaler as the source of the pheromone." From an evolutionary theory viewpoint this is a weak argument, since if it was not in the female interest to receive a message they would have evolved not to receive it, or not to let it affect their behavior. A signaling system must be in the interests of both parties. The purpose of this paper is to provide a plausible reason for the emergence and continuation of female sensitivity to androstenol, and for the male ability to emit it.

It does appear that human males are designed to emit androstenol, or closely related pheromones (Kohl & Francoeur, 1995; Stoddart, 1990). It is produced in apocrine glands that are typically at the follicles from whence hair grows, especially under the arm and in the public regions. They appear designed to wick up the odors from the apocrine glands and distribute them to the atmosphere. This is shown by the axillary hairs being associated with more apocrine glands than hairs in other parts of the body. The musky odor produced from the axillae is caused by the presence there of androstene and 5a-androstenol. Androstenol is known to be a sexual pheromone in the pig which causes the sow to assume the position required for mating. Men produce much more androstenol than do women in their urine (Brooksbank, 1962) and more androstenone in their axillae (Bird, & Gower, 1982, as cited by Stoddart, 1990, p. 66).

While some human hairs can be argued to serve various purposes, such as protecting the head from the sun, or sexual signaling, the underarm hairs are not located to keep us warm, nor to protect us from the sun, or even to be visible at a distance. The only plausible purpose appears to be facilitating emitting odors or pheromones. They do this efficiently. Only one in ten shaved armpits are described as odorous 24 hours after washing, while nine out of ten unshaved armpits are described as odorous (Shelley, Hurley & Nicols, 1953, as cited in Stoddart, 1990, pp. 66-67). The concentration

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of hairs here, served by multiple apocrine glands, must be an adaptation for the production and dissemination of odors.

One evidence for human pheromones is menstrual synchrony, by which human female coordinate their cycles (for references see Miller, 1998b, in this journal, and McClintock, 1984, or Stern & McClintock, 1998).

Humans do appear designed to receive pheromones, since they have functioning odor detection and apparently functioning vomeronasal systems (Berlineret al. 1996; Garcia-Velasco& Mondragon, 1991; Monti-Bloch et al. 1994, Moran et al.1991, Monti-Bloch, Jennings-White, Berliner, 1998, Monti-Bloch 1999). Two of the substances reported in (Monti-Bloch & Grosser, 1991) as affecting the human vomeronasal organ were subsequently revealed (Jenning-White, 1995) to be estra-1,3,5(10), 16-tetraen-3-ol (estratetraenol) and androsta-4, 16-dien-3-one (androstadienone). The latter affects females more strongly and is structurally related to androstenol. It is present in male sweat (Gower et al. 1994). It has been recently shown to affect female mood favorably (Monti-Bloch, Grosser, Jennings-White, & Berliner, 1998). It has also been shown that only some of the various apparently similar androstanes affect the vomeronasal organ (Jennings-White et al. 1997).

Humans are not noted for their ability to detect odors over long distances. Thus, I would propose that in considering human pheromones that we abandon the analogy from insects and some mammals of an effect operating at long distances, such as the attraction or identification of mates. Humans can clearly recognize and identify mates, potential mates, and children at long distance by sight and sound. A pheromonal mechanism would add little to what can be accomplished by these other means.

However, pheromones in many mammals do play a role in reproduction, most prominently in attracting a mate, and preparing the mate for intercourse and reproduction. I would suggest that the human pheromone, androstenol, (and possibly other human pheromones) plays a role in reproduction, but that this role relates to the formation and maintenance of the pair bond. In essence, detection of a pheromone emitted by the opposite sex, and only detectable at very close range (such as occurs when sleeping together) is a sign of the presence of a mate. The pheromone serves to

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promote behavior that leads to the formation or continuation of a pair bond, or to reproductive behavior which is optimal in the presence of a pair bond.

The experiment described was highly artificial with the exposure delivered via a necklace. Of course, in the prehistoric conditions in which humans evolved, they would not have been exposed via necklaces. If human females are affected by androstenol in nature, one would ask where and how they got sufficient exposure for a small but functional main and accessory olfactory systems to detect it. The obvious answer is in sleeping with their mates. Bonded humans regularly sleep together, frequently with the female wrapped in the arms of the mate in a position that places her nostrils near the axillae of the male.

Cowley & Brooksbank demonstrated that females exposed to androstenol engage in more frequent, longer duration, and deeper interactions with men. In the experiment the females met a wide range of men, including family members, men on public transport, in halls and rooms. Because the women were wearing an androstenol emitting necklace they would have been under the influence of the androstenol when they met any of these men.

However, in natural conditions their exposure to androstenol would have probably been during the night. The exposure would have disappeared when the female arose and went about her daily business. The only male who benefited from her greater "approachability" would normally be either her mate, or a mate to be.

Much of the androstenol induced interaction would be what is referred to in our society as "pillow talk", discussion with the mate or potential mate while lying with him. It is easy to imagine how such pillow talk creates and strengthens the bond between male and female.

It may also be noted that pillow talk may help the female evaluate the male. Engaging in much longer and deeper interactions than normal, she may learn things from the conversation that help her to evaluate the male's suitability as a long term mate, and his willingness to assist in child rearing.

It is useful to ask if a system by which males are designed to emit androstenone to promote pillow talk, and females to receive it, would be evolutionarily stable. By evolutionarily stable is meant that

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individuals of either sex that lacked the trait or the ability to respond to it would enjoy less reproductive success than ones that had it (Maynard-Smith, 1982).

First consider a female who did not respond to androstenol. Engaging in less interaction with her mate, she might form a less strong pair bond, and would have less male assistance in raising her children. Identifying and attracting a male to provide sperm is not a female problem in humans (hence pheromones probably do not play a role), but inducing one to help raise the children is a problem (Symons, 1979; Hrdy, 1981; Ridley, 1993). Thus, anything that promotes a pair bond probably helps the female.

Also, a female who did not experience an increased desire to interact with a specific male would have less information about him. Such information assists her in deciding whether to form or maintain a pair bond with him. The androstenol promoted interaction should facilitate the bonding between the male and female, which is critical to female reproductive success.

Consider a female who interacted very readily with all males, regardless of whether she was cuddling or sleeping with them. Such interaction would be very time consuming. Thus, females who did not adjust their willingness to interact to whether the male had already been granted sufficient access to cuddle or sleep with (which normally indicates either the existence of a pair bond, or a pair bond in the process of formation) would waste much energy, and increase their risk of forming inappropriate pair bonds.

The same argument would apply to a female who had a very sensitive system for recognizing androstenol or other pheromones. She would be affected by the pheromones emitted not only by mates and potential mates but by other males in the vicinity. She would lose the benefit of disproportionate interaction with mates and potential mates. Thus, this theory can explain not only why females have retained the ability to be affected by the pheromone, androstenol, but also why their sensitivity to it is relatively weak (at least when compared with the sensitivity exhibited in other species to pheromones).

What about males? Consider a male who did not emit androstenol, or who had relatively few hairs to disseminate it. Females would not be induced to interact with him when he cuddled

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or slept with them. He would be less likely to form the sort of strong pair bond that gave him continued sexual access and which led to reproductive success.

Thus, a role for androstenol as a human pheromone that promotes "pillow talk" seems to be one that could emerge and survive. Since in other species pheromones signal between males and females it is likely that it earlier played a role in the relations between males and females. Once pair bonding became important in humans, this mechanism could have been adapted, merely by forming a few new connections in the brain, for differentially interacting with mates and potential mates. Meanwhile, other uses for the mechanism might have gradually disappeared if they were no longer needed, or were counterproductive.

Admittedly, there is an alternative interpretation to the Cowley & Brooksbank finding. Since the subjects' reports on interaction were based on questionnaires completed after the necklaces were turned in, and it is hard to recall all interactions, it is possible that the number of interactions were not affected by the androstenol, but their ability to recollect interactions was. This explanation is unlikely. Since forgotten interactions would probably be the least in depth and shortest ones, such as a casual greeting, remembering more interactions would lower the average depth. However, androstenol raised the depth. However, even if the effect was due to androstenol affecting memory for interactions with males, the effect could still be part of the pair bonding mechanism. Something that improved a woman's memory for interactions with a man she slept or cuddled with would be a most useful adaptation. It would assist her in choosing whether to continue the relationship, and increase her retention of information that would assist her in maintaining the pair bond.

Other effects of androstenol and related compounds on behavior can be interpreted as adaptations that facilitate pair bonding (Cowley, Johnson, & Brooksbank, 1977; Filsinger et al., 1984; Kirk-Smith et al., 1978). For instance, Cowley, Johnson, & Brooksbank (1977) found that women rate men more positively when exposed to androstenol. Benton (1982) found that women exposed to androstenol on the upper lip rated themselves as more submissive around mid-cycle, a feeling that encourages impregnation. However, women in the fertile

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period of their cycle prefer the scent of symmetrical men, whichis interpreted as an evolved preference for sires with good genes." (Thornhill and Gangestad, 1999. Gangestad and Simpson, 2000).

Of course, androstenol and other pheromones released by males could have other purposes. The author has argued in another article in this journal (Miller, 1998a) that the raising of female fertility by the proximity to a human male is a logical adaptation. It serves to increase the probability that women become pregnant by "dads" who will stay around and assist in raising the offspring, rather than by "cads" who will, after a "one night stand," abandon the woman. If one of the effects of physical proximity is to promote fertility, perhaps by changing the hormonal environment so that implantation was more like, it would be a useful adaptation.

Several, otherwise puzzling effects in the literature could be explained such as the tendency for women who regularly saw men to have more menstrual cycles of a length more conducive to fertility (Cutler et al., 1991; Cutler et al. 1979; Cutler et al., 1980; Cutler et al., 1985; Cutler, Garcia, Huggins,& Preti,1986), the ability of an extract from males to affect the female cycle (Cutler, Preti, Krieger, Huggins, Garcia, & Lawley, 1986; Russel et al., 1980), exposure to men influencing the occurrence of ovulation in women (Veith et al., 1983), and the ability of sexual intercourse to increase the probability of success in gamete intrafallopian transfer (Marconi et al. 1989). However, discussing this possible effect of male pheromones is the subject of another paper (Miller, 1998a).

Since that paper was written it has been shown that male levels of testosterone are lowered by exposure to a steroid very similar to a substance emitted by human female skin (Monti-Bloch, Diaz-Sanchez, Jennings-White, & Berliner, 1998). Lower levels of testosterone would reduce the risk of male aggression that might adversely affect the pair bond, reducing the reproductive success of both partners. Such an effect is very consistent with what was proposed in the earlier paper (Miller, 1998a).

It has also been shown (Monti-Bloch, Grosser, Jennings-White, & Berliner, 1998) that one substance detected by the human vomeronasal organ (androstadienone) and chemically similar to androstenol, has an effect on human female mood (relaxing) which plausibly could be interpreted as making the female calmer in the

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presence of a male (probably a mate). Emitting such a pheromone would be in the interest of males. Being affected would be in the interest of the female. These two would make the pheromone effect evolutionarily stable.

Further support for the idea that there are human pheromones is supplied by the recent evidence using functional magnetic resonance imagining that brain activation (in the hypothalamus, amygdala and cingulate gyrus-related structures, which are related to reproduction and emotions rather than conscious thought) can be produced by a chemical (one of the pheromones being studied by Monti-Bloch, Berliner, and colleagues and discussed above) that cannot be smelled (Sobel et al., 1999).

Implications for Further Research

As reported, Cowley & Brooksbank just have described an interesting effect. However, if female exposure to androstenol serves the purpose hypothesized, it could be important. For instance, if human pair bonding is promoted by exposure to axillary pheromones, perhaps antiperspirants serve to reduce a useful form of chemical communication from males to females. This seems an important enough question to justify further research.

An obvious next step in the research is to do a double blind study in which the males in a married couple use either an antiperspirant or a deodorant on a random basis and then fill in questionnaires on the extent of their interactions (conversations), both that morning and at other times. Since there is a good chance that female emitted pheromones affect males, a similar experiment should be done with females.

Conclusions

Although, Cowley & Brooksbank have provided evidence that female exposure to androstenol increases female interactions with males, including many casual interactions with males met before arrival at school, in nature the only male interactions promoted would be with mates or potential mates. The observed behavioral response is probably a mechanism for promoting bonding with mates, and evaluation of potential mates. Recognizing an evolutionary purpose or the female reaction to androstenol makes one feel more confident

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that the observed effects are real.

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The British Martyrs, Aaron and Julius

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The paper explains the sub-Roman British 'martyrs' cult of Aaron and Julius in terms of the syncretic adaptation of pagan traditions, suggesting close parallels from contemporary Gaul and a direct link with these in the person of St. Germanus of Auxerre. It suggests that 'Iulius' (and other 'saints' of similar name) represents a continuation of the Indo-European tradition of divinities in **dyew-*, while Aaron (and other 'saints') represents the adaptation of ultimately non-Indo-European traditions that have left their trace in the toponymy. Derivatives of our syncretic figures and their antecedents (or close relatives thereof) are identified in medieval Celtic literature. An attempt is made to correlate the evidence for cult traditions in the hagiography, other literary sources, and the toponymy: thereby explaining certain facets of all of these.

Key Words: St. Aaron, St. Julius, Celtic paganism, early Christianity, Indo-European mythology

It has long been the tendency to accept Britain's three recorded Christian martyrs as genuine without serious question.[2] The degree of certainty might seem rather surprising, given the limitations of the evidence and the considerable number of, more or less certainly, 'bogus' martyrs from parts of the Christian world for which our records are much better. The evidence in the case of 'Aaron and Julius' – to whom the discussion here will be limited – rests almost entirely on the single mention in the *De Excidio* of Gildas, conventionally believed to have been written about 540 but in any case not likely to pre-date the sixth century[3]:

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[2] E.g. Thomas 1981: 47-50; Boon, 1992; Salway 1981: 721; W.H.C. Frend, quoted in Salway, 718, is more cautious : "...allegedly martyred at Caerleon". For the likely nonexistence of other British martyrs, Delehaye 1931a.

[3] *De Excidio 10* in Winterbottom *1978: 19, 92.* For the date, Sims-Williams 1983: 3-5.

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> God therefore increased his pity for us .... as a free gift to us, in the time (as I conjecture) of this same persecution, he acted to save Britain being plunged deep in the thick darkness of black night; for he lit for us the brilliant lamps of the holy martyrs. The graves and the places where they suffered would now have the greatest effect in instilling the blaze of divine charity in the minds of beholders, were it not that our citizens, thanks to our sins, have been deprived of many of them by the unhappy partition with the barbarians. I refer to St. Alban of Verulam, Aaron and Julius, citizens of Caerleon *(Aaron et Iulium Legionum Urbis cives)* and the others of both sexes who, in different places, displayed the highest spirit in the battle-line of Christ.

A clue to the quality of Gildas's source of information, here, is given by the fact that he had to make a guess about the actual date of the martyrdom: moreover this guess is now generally considered to be incorrect, because the persecution of Diocletian to which he attributes their martyrdom is now considered not to have extended to Gaul and Britain.[4] Gildas was clearly basing his guess on what he had heard and read about the history of Christianity in the rest of the Empire: we might well wonder to what extent the cult of martyrs in Britain, in general, owed its existence to imitation of the model provided by other parts of the Empire (or ex-Empire) rather than to deep-rooted native Christian tradition. The reference to 'the graves and the places where they suffered' suggests that it is the current, or recent, existence of local cults on which Gildas based his account of British martyrs: not on any kind of historical record.

This mention in Gildas, it is true, is backed up by the record in the Book of Llandaff (Rhys 1893: 225) of a *territorium fanctorum martirum Iulij et aaron* at Caerleon (a Welsh name equivalent to *Legionum Urbis)* in a twelfth century version of a charter from the ninth century (Davies 1979: 121). This has been thought likely to have reference to Bulmore, over the river from Caerleon, two miles to the East: it might be seen as a not un-typical location for an 'extra-mural'

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[4] Eusebius, *Historia Ecclesiastica 8.13;* Lactantius, *De Morte Persecutorum 15;* Levison 1941: 344; Thomas 1981: 48.

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church, built over the martyrs' grave.[5] However this might also be a likely place for a rather artificial cult; based on the mention of the martyrs of *Legionum Urbis* in the work of the revered saint, Gildas, to have been established in the ninth century: the alternative (Smith 1979: 6-7) 'Carlegion' (or *Legionum Urbis* in Latin), Chester, will not have been chosen because that was now in the hands of the Saxons. In any case the existence of the cult, here at Caerleon, in the ninth century cannot be proven to be independent of the mention in Gildas, while the existence, anyway, of a cult-centre, here, from the time of Gildas would not be proof of an actual martyrdom in the very early fourth century, at the latest, and more likely the third.

That the creation of 'bogus' martyrs throughout the one time Empire was more or less endemic has been well demonstrated (Jones 1964: 958; Delehaye 1933: 70). This was especially the case in the West which Christianity reached only later: Griffe (1964: 1, 130-3) has pointed out the rather small number of genuine martyrs in Gaul. In Britain the number of Christians before Constantine (and therefore martyrs) is likely to have been proportionately smaller again (Thomas 1981: 44). 'Bogus' martyrs were created in various ways: tombs were wrongly ascribed to martyrs (Delehaye 1933: 88-9), locally notorious figures were described as such (*Vita Martini* II) and the 'localisation' of translated relics led to 'duplications' (Van Darn 1988: 80-1, 87). The Christianisation of pagan cults is more controversial (contrast Jones 1964: 961 with Saintyves 1907) but seems likely to have happened (e.g. Wood 1994: 74) and, incontrovertibly later, a 'saint' like 'Bridget' clearly represents a continuation of the cult of 'Brigantia'. Immediately post-Roman Britain, moreover, will have represented an extreme case of the disruption and confusion that accompanied the 'decline and fall' of Rome – with a rapid and complete collapse of the economy and urban life – so that the conditions for the preservation of authentic tradition will have been

[5] Boon 1992: 21-2, note 10. This is on the basis of the bounds in the charter, while a 9/10th century fragment of an interlaced cross-head was found near here and there was a chapel to St Alban on the hill above: the cult of Aaron and Julius attracted that of the other martyr, St Alban, to Caerleon by 1143 (Levison 1941: 339-42) so the chapel may have been earlier to all three martyrs, originally to Aaron and Iulius, alone.

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extremely poor while any number of bogus cults might have arisen out of the confusion, perhaps most of the many martyrs mentioned by Gildas: and quite possibly those he mentions by name, whose cults we must assume were dominant in his time.

A more specific point to make about the likelihood of the actual existence of our two martyrs is that 'Aaron' was in fact a very rare name in antiquity for Jews and non-Jews alike. It has been suggested that it was an adopted 'baptismal' name, in this case (Boon 1992: 13), but if so, this is the only record of its use in this way. The assumption of a genuine martyrdom will most likely continue to be made, however – if only because 'nature abhors a vacuum' – until someone produces a credible alternative origin for the cult. It is my aim, here to provide that – and in so doing to cast some light on some broader issues, as well.

The post-ninth century cult of 'Aaron and Julius' is not very impressive. It seems to be limited to Caerleon and it's immediate environs to which it attracted a complementary cult of Alban by the twelfth century (Gould 1907:1, 103-4; my note 4). This development reflects the influence of the mention in Gildas which associates the three together and this may also have been at work in the association of a Breton St. Aaron with St. Alban. Some forty kilometers West of St. Malo there is a village of 'Saint Aaron' close to a village of 'Saint Alban' and this and other localised manifestations of a St. Aaron in Brittany (Duine 1918: 380-1; Corson IV, 262-3, VI, 338) have been thought to represent the British martyr (so Loth 1910: 7). The most important Breton St. Aaron, the original saint of the island of St. Malo, was also associated with St. Alban, by his feast date (recorded in the 15th century: Duine 1905: 71, 73, 79) and on this basis seems to have been perceived as the 'same' saint as the other 'Aarons'. However, he was recorded in the ninth century, in the early lives of St. Malo, as an independent figure: a sixth century ascetic rather than a Roman-era martyr (Lot: 1907). The only recorded feast-date, meanwhile, for the British martyrs (recorded from the l6th century: Gould 1907: 1, 103) looks highly artificial since it is the same as that for the Biblical Aaron, and so would seem to be based on that. In fact Levison (1941: 343, note 11) noted that this feast date, the first of July, was not only appropriate to an 'Aaron': it also occurs on the first of the month of 'Iulius', the name of which echoes that of our

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second martyr.

It is this fact that should perhaps have made Levison think again. The association with the name 'Aaron', alone, would have been enough to attract the feast-day of our martyrs' cult, to this date in the calendar. Was it not rather a remarkable coincidence that the biblical Aaron was celebrated on the first of the month of 'IULIUS', in the first place? Note that it was the first, or 'Kalends', of the month – not just any day in July. If one takes the time to think it over then the less and less likely it begins to seem that this date for the Biblical 'Aaron' and the association together of an Aaron and a Iulius in our British cult are entirely coincidental. The date for the Biblical Aaron is not shared by the Eastern church and seems to originate with the *Martyrologium Hieronymianum* (Delehaye 1931b: 345-7; Duchesne 1894: 85). This was compiled – or revised – probably not long before A.D. 600, at Auxerre. Some kind of influence from, or connection with, our British cult might not seem impossible, given the known presence there of another British cult, that of St. Alban (Picard 1992: 56), and of the links with Britain reflected in the career of St. Germanus. There are, in fact, other hints of a continuation of links between Auxerre and the Celtic British Isles, the place of Auxerre in Patrician hagiography, being one of them (Fleuriot 1980: 154; Bieler 1979: 70-1, 124-5).

But if there is a connection it is not at all easy to envisage what it might be: it hardly seems possible to see one of Gildas's martyrs evolving out of the name for a month, while it does not seem very realistic to see a British cult influencing the feast-date of this Biblical figure in this particular way, either. The best bet would perhaps be to look for some, deeper, root-cause that might explain both phenomenon.

Our starting point might be Germanus, as embodying the most conspicuous link between Auxerre and Britain: Germanus does not show any signs of an association with a cult of a martyr Aaron or Julius but he comes quite close to the latter, one could say, in the sense that he is associated with the cult of a martyr 'Julian'. This is 'Julian of Brioude' whose cult in the Auvergne, rose to prominence during the fifth century (Sidonius, *Carm.* 24. 16-17; Ep. 7. 1). It was Germanus, apparently, who was responsible for instituting the observance of his festival on a regular date, so the late sixth century

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'Life of Julian' by one of his devotees, Gregory of Tours, tells us (*Vita Juliane*[6] 29). We might suspect that Germanus actually played an even greater role, than Gregory makes apparent, in instituting or 'regularising' this cult which seemed to be the Christian 'answer' to an important pagan cult at Brioude (*VJ* 5; Van Dam 1993: 167, 42). Julian was supposedly a martyr of circa 300 although neither Gregory or a probably late fifth century Passio (*Passio Juliane*) offer any very credible story about him. They do, however, link him with another martyr: something that parallels the way our Iulius is linked with another martyr, Aaron. Julian was supposedly the friend of Ferreolus, the martyr of Vienne, before he fled that city to meet his own end at Brioude (*VJ* 1; PJ 2-3). This association went back to circa 473 when Bishop Marnertus of Vienne claimed to have found the head of Julian buried with the body of Ferreolus (VJ 2; cf. Sidonius Ep. 7. 1): this story, which savours of the old Celtic cult of the severed head may have been based on the discovery of an actual 'tête coupée' burial, but if so that burial[7] was interpreted in the light of a tradition that already associated the two martyrs together.

'Ferreolus', of course, does not sound anything like 'Aaron' (as we might like it to): it is evidently a diminutive based on the Latin *ferrus*, 'iron'. Ferreolus does, however, display a suspicious affinity for the Rhone: his original church (over his 'grave') was on its banks and it may be no coincidence that his probably fifth century Passio (ASS Sept. V, 764-5; Griffe: 1964: 155) involves him in a swim across it and then a further journey along its bank as far as the river Gier which joins it above Vienne. The sceptic might suspect that we have here some Christianised version of an ancient cult associated with the river Rhone.[8] On this basis, anyway, we might hope that the name of the

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[6] *VJ PJ*, in Krusch 1885: 562-84, 879-81; trans. in Van Dam 1993: 162-95, 196-8.

[7] E.g. Green 1986: 130- 1. Cf also "the association of heads with water, spring and well cults" (p. 166) with VJ 3, the washing of Julian's head in the spring.

[8] Cf. St Genesius of Arles (Gregory of Tours' *Glory of the Martyrs* in Van Dam: 1988, 91-2; Attwater 1956: 111, 591-2; Griffe 1964: 153-4, 166-7; Delehaye 1933: 348-9; the earliest record is in Prudentius, late 4th c.) who also swims across the Rhone (in his probably 5th c. Passio: p. 135 in *ASS* August V, 123-36). The story told of Ferreolus might be based on this, but might also represent basic

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river involved might offer us some clues, but the name 'Rhodanus' (Dauzat 1978: 778) does not seem at all helpful to us. But this may not have been the original, or only, name of the river. The names of its two main tributaries (above and below Vienne) both contained the element *ara* and one of them consisted of this element twice over. Thus, in Caesar, the 'Isere' was the 'Isara', while the Saone was the 'Arar' (Dauzat 1978: 56, 87). Further upstream is the 'Arve' (Dauzat 1978: 20) while downstream the name of the most important town on its banks, 'Arelate', 'Arles' (Rostaing 1950: 58-61) may contain this element, while in the Bouches de Rhone' we find the 'Aren', anciently the 'Arannus' (Dauzat 1960: 135). In. fact this common river-name element, *ar(a)-* is particularly concentrated in the region of the Rhone/Saone basin and Albert Dauzat (1960: 131-41) believed it to be of pre-Indo-European origin, belonging to the most primitive strata of toponymy: he noted that it occurs with suffixes, compounded with other elements, and as a suffix, itself, as well as alone and, as we have seen, even recombined with itself (cf. Rivet 1979: 389; Rostaing 1950: 56): it is particularly significant that this latter form, probably signifying 'great river' was used for a major tributary of the Rhone.

This name *Ar* begins to raise interesting possibilities for us: it seems to offer some kind of rapprochement with our martyr's name, 'Aaron'. River names formed from it include 'Aranus', 'Aronnu/a' and 'Arona', with the *-ona* suffix, typical of hydronyms that are also

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similarities in the nature of the two cults: the relatively early record of Genesius is not a guarantee of his 'genuineness' as a martyr while the cult of an actual martyr might have become confused/associated with an earlier dominant one associated with the Rhone. Genesius, like Ferreolus, originally had a church on the banks of the Rhone (Delehaye: 349; Griffe: L 166-7; ASS, p. 135, c. 6) while both of his miracles as described by Gregory (c. 68) involve the Rhone. It might be significant that it is that river that is involved in both cases of 'nautical' martyrs. For the association of euheumerised deity with river, expressed in this way cf., e.g., Horatius Cocles' dive into the Tiber (e.g. Grant 1971: 181-5). One interpretation of Genesius's occupation as 'notary' might be that it derives from the association in myth of the fluidity of water with the fluidity of speech - and its written equivalent. For an association with the spoken variety cf. 'Genesius the actor' (ASS August V 119-23; Delehaye: 462-5; Van de Vorst, *Analecta Bollandiana* 29, 1910: 258-69), one of a rather large number of doubtful duplicates (Van Darn 1988: notes 80, 88; Delehaye: 348) generated by the cult.

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theonyms.[9] Moreover that there was indeed a divinity of *Ar-* type associated with the 'Arar' river name is suggested by the appearance of the name 'Arus' on coins of the Segusiavi, the tribe that occupied the region around the Southern part of that river, to the North of Lyons/Lugdunum. This name was evidently appropriate to a god or divine hero since it appears together with the image of Hercules: showing the typical assimilation of a native, to a Romano-Greek deity. The name 'Arus', here, does not, it is true, seem to be, in any obvious way, appropriate to a divine personification of the river itself; but given the location of the Segusiavi it is hard not to suspect some kind of link with the river name. In fact Duchalais, in his discussion of the coin, linked Hercules in this context with water: specifically thermal waters, which exist in the region, and in which Hercules is said, in various accounts, to have regained his strength with the goodwill of their presiding goddess (Holder 1896: 1, 230-1; Duchalais: 129-35, plate 2).

In fact the *Ar-* name also offers a credible raison d'être behind the Iulius/Aaron pairing itself This, however, will involve us in an exploration into some of the associations of the other of the two names here: 'Iulius', and its derivative, 'Iulianus'. The Latin *nomen gentilicium,* 'Iulius' (equivalent to Oscan 'Iuvilus' and 'Diuvilam') is to be analysed, 'iou' plus 'il', where the 'iou' (as in the other Latin names 'Iouinis' and 'Iouianus'), is from the root *dyew* (ultimately from **dey-*, 'to shine') that is found in divine names such as 'Iupiter' *(Diespiter)* and Greek 'Zeus', as well as in words for day like Latin *dies,* with a close relative found in words for 'god' like Latin *deus. *Dyews,* then, is the (reconstructed) name of a 'sky god': the archetypal and best attested Indo-European deity (Meillet 1967: 328-9, 170-1, 174-5; Eliade 1979: 189). 'Iufius', therefore, would seem to have originally been a 'divine', or 'cult', name, representing one of the many devolved forms of that deity.

The family name, of that most famous Roman, Julius Caesar, seems to have been associated with the cult of Apollo and since that

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[9] Dauzat 1960: 134, 140; Nicolaisen 1957: 230, note 53. For *-ona* see Nicolaisen 1976: 178-9; Dauzat: 1960: 118-2 1; Hamp, *Eriu* 29, 153. For other examples of a close relationship between hydronyms, pagan divine names and saints' names, see Saintyves 1907: 346-50.

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god was closely associated with the sun (at its height in the month given Caesar's name) this may reflect the fact that the relationship between the 'sky' and 'sun' god was always a close one (e.g. O'Rahilly 1946: 58). The patronage of the cult of Apollo by Augustus Caesar seems likely to have been inspired by more than the presence of a temple of that god at Actium (e.g. Grenier 1948: 187-8), since it was a cult associated by Servius, in his commentary on Virgil's Aeneid, with the caesarian birth of the first 'Caesar': parallel to the similar birth of Aesculapius, son of Apollo (Fowler 1899: 182).

It is tempting, furthermore, to suspect there might be some connection between 'Asc-lepius' and the 'Asc-anius',[10] who was identified with 'Iulus', the legendary founder of the 'Iulian' family. 'Iulus/Ascanius' was the son of Aeneas, and ruler of the city of 'Lavinium', before moving to 'Alba Longa' *(Aeneid I,* 267-71; Livy 1.3). Lavinium (from which the cult of 'Sol Indeges' at Rome probably came: Grenier 1948: 87, 135) was the conduit by which much early Greek influence reached Rome. It was home to an important cult of Aeneas, for instance, who was worshipped there, as 'Pater', or 'Iupiter', 'Indeges' (Livy 1.2). As T. J. Cornell (1995: 63-8) comments, "This must imply a secondary elaboration of a pre-existing cult, that is to say, Aeneas was at some stage equated with a local cult". Parallel to this, the two names for his 'son' might derive from a Greek equivalent for an original Latin cult-figure: or at least it was the divine associations of their name that allowed the Julian family to promote the parallel identification of 'Iu-lus' with 'Ascanius' (Grant 1971: 94).

The association between the name 'Iulius' and a sun-god is significant when we consider that, in later, medieval, times a name often regarded as equivalent to 'Julian' in the Celtic lands, was 'Sulien' (Doble 1970: V, 122; Gould 1907: IV, 203, note 2), where the root *sul-* derives from Latin *sol* (e.g. Fleuriot 1980: 75). This was especially the case with the various Celtic saints of that name, or whose name contained the root, *sul-*, who were re-interpreted as a

[10] The 'A(e)sc-' in *Ασκληπιός*, 'Aesculapius' and 'Ascanius' might be connected with a pre-Indo-European word for a type of Oak (with cult associations?): Meillet 1967: 13: *aesculus*; Chantraine 1983: 125: *ἄσκρα*; Hubschmid 1963: 11, 80.

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'Julius' or 'Julian' or equated with a saint of that name (e.g. Doble 1970 V, 122; Gould 1907: IV, 204-5). It is tempting, moreover, to suspect some kind of continuity between these names and earlier Celtic names in 'Sul-' especially the divine names 'Sulis' (as in 'Aquae Sulis' = Bath) and 'Suleviae', with which the personal names were closely associated.[11] The link cannot be direct since original Celtic (as against borrowed Latin) 's' gave later 'h' (Jackson 1953: 15), but, nevertheless, the 'Sul-' in all these names was cognate with Irish suil (meaning 'eye' but from a word for the 'sun') and ultimately with Latin sol as well as a host of other Indo-European words for 'sun' (Rivet 1979: 255-6; Vendries 1959: S-201). Furthermore there is some evidence that sul- was often Latinised as *sol-* (Rivet 1979: 320-1) and we might anyway suspect that the native divine name 'Sul' will tend to have become assimilated to the divine name 'Sol', given the importance of the cult of a divine 'Sol', especially 'Sol Invictus', in the late Roman period. Such an assimilation offers an explanation for what might seem inherently likely, namely some kind of continuity between the old Celtic 'divine', and new Brittonic 'saints', names in 'Sul-'.

It is true that the equivalence of names in 'Jul-' and 'Sul-' is to be seen primarily in terms of sound-assimilation. It is paralleled by the way that the Welsh St. 'Silin' was regarded as equivalent to the Continental 'Gilles' (Giles), and, in fact, several names that were borrowed into Welsh developed their own Welsh forms in which an original 'j', or soft 'g', or 'ch-' (sounds that were not found in Welsh) were replaced by an 's' ('Sior' for 'George', 'Sion' for 'John' etc..). It is difficult to know at how early a date this kind of sound-substitution might have begun but, in any case, original British-Latin 'Iulius' developed into Welsh 'Iwl': the initial 'I' never developed the different kind of pronunciation (like the sound of the 'J' in modern English 'Julius') that it did in Gaul and on the continent. There the 'I' of 'Iulius/an' had developed, from the original pronunciation [i] to [ž] in Vulgar Latin, by the first to third century and [dž] by the fifth to sixth century (Jackson 1953: 344, note 2). So it might have been

[11] Green 1986: 'Sulis' 154-5, 255-6; 'Suleviae' : 79-82, 84, 155. For early Celtic names in *Sul-*, Evans 1967: 471-2. For two people named 'Sulinus' from 'Aquae Sulis', one of whom dedicated two altars to the *matres Suleviae* Birley 1979: 130.

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possible, in theory at least, for there to have been some kind of assimilation between names in 'Sul-' and 'Iul-' if a continental 'Julius', or 'Julian' was involved - and in fact we have already begun to suggest that, by hinting at a link between the British 'Iulius' and the 'Gaulish' 'Julian' of Brioude. The latter might conceivably have been regarded as equivalent to a name in 'Sul-' by (the remaining) Celtic-speakers in Gaul. In this context a name in 'Iul-' might have not only sounded similar to a name in 'Sul-', but might also, as we have seen, have had parallel cult associations. These might have appealed to the more conservative religious sentiments of the rural populace while on the other hand it would have been a sufficiently common and innocuous name so as not to offend mainstream urban Christians. While a native name in 'Sul-' may previously, and most obviously, have been Latinised as 'Sol-' it may be that 'Iulius/an' was the most obvious name to substitute for it in order to convert it – and the cult it was linked to – into something acceptable to now Christian Latin-speakers.

In any case, the association of the name 'Iulius', or 'Iulianus', with a name 'Sul', for the sun, is particularly enticing in the context of our discussion, above: because we have suggested that *ar-* is an element closely associated with rivers and water. The combination of 'sun' and 'water' is surely an eminently feasible duality to find at the heart of a Celtic-pagan system of beliefs. Sun-worship – and the cycle of 'sun, rain and consequent fertility' – seems to have been at the heart of the religious beliefs of North West Europe during the late Bronze Age, and underlies subsequent Celtic and Teutonic religion (e.g. Green: 1986: 39 ff., 147, 164-5). The fact that such a sun-water duality was associated with 'Sulis' is implied by the etymological associations of that name and the location of her cult centre at the hot springs of 'Aquae Sulis'. In fact given the strength of certain conceptual associations in Celtic and other pagan religions it may be that a 'sun' god would automatically be associated with water (and, with this, no doubt, fertility) and vice-versa so that it may well be that all of these associations were present with both of our presumed deities: in that case their pairing together might represent the matching up of 'equivalent' deities from different religious (ultimately ethno-linguistic) strata. In this case, specifically, we would have a pre-Indo-European deity, *Ar-*, and its Indo-European 'equivalent' of

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typical *dyew-* type. We could compare the association of these two types together in other contexts: Iupiter with Mars, for instance, in the Roman pantheon (Meillet 1967: 388) or Zeus and, say, Artemis, in the Greek (Chantraine 1983: 116-7): albeit these are not closely matched pairs, as such. It is likely enough that our view of the complex and overlapping associations of our two figures will be a necessarily foggy and simplified one. Nevertheless, we can begin to see, here, some kind of credible explanation for the association together of 'Iulius' and 'Aaron' as expressed both in our British cult and in the choice of feast-date, made by the Western church, (at Auxerre ?) for the Biblical Aaron, on the basis that both of these represent the influence of strong indigenous pre-Christian traditions.

We would propose assimilation of our divine name in *Ar-*, via an *ar-ona* form, or one with an ordinary Latin suffix, to the well known Biblical name, 'Aaron' which would have represented an ideal Christian substitute for the pagan name. We still have the problem, of course, that our 'Aaron' is associated with a 'Iulius' in Britain and not with 'Julian' and the Rhone, in Gaul: the name 'Ferreolus' seems to present something of a problem to our scheme. We can note, though, that Dauzat (1960: 115-8) also draws attention to an Indo-European river-name element *vara,* which had a variant *vera,* or anyway became that in late Latin. It is probably related to Latin *urina* and means just 'water' but since it is found widely distributed in river names[12] like *ara,* and sounds very similar, one can perhaps envisage a degree of assimilation, or confusion, between the two (and see further below). It might be, anyway, that we have here a clue to a pagan name that stands behind 'Ferr-eolus'. The latter part of that name might correspond to a native diminutive or suffix. On the other hand it looks very much that ancient tradition at Vienne involved a pair of deities – hence the need for two 'martyrs' – while the Passio of Ferreolus seems to show that the earliest tradition did not involve Julian, the connection with him being a later development associated with the 'inventio' of Mamertus. Might it be, then, that the original choice of martyr's name, itself, reflected somehow the tradition of a cult that involved two figures? So that Ferreolus represents two

[12] Rivet 1979:485-6; Nicolaisen 1976:180-3; Dauzat 1978:93-4; Krahe 1964:38-40; Vennernan 1994: 222 (distribution map).

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elements, *Vera, plus 'Iulius'* or a similar-sounding native name, run together and assimilated to the acceptably innocuous Latin 'Ferreolus'.

Once again we might have a name that could have meant different things to different people, or that originated in the process that turned a pagan cult into an acceptable Christian one. It might be significant that the presence of the Ferreolus cult at Brioude came in the form of an oratory near the spring where the head of Julian was said to have been washed (*VJ* 25-6; Delehaye 1933: 344): there would seem to be no good reason for its location here, rather than actually in Brioude, unless there was some special link between the cult of Ferreolus; and water. At any rate this theory offers some kind of explanation for the rather peculiar relationship between Julian and Ferreolus while also showing how it might parallel the one between Aaron (<*ar-on*) and Julius.

Another problem might seem to be the rather special mention of a named river, the Gier *(ad Jarem)* in the Passio story: if there is no factual basis in the story than the place in it, of this river, is likely to have some special significance. Perhaps the earliest site associated with the cult was on its banks but if so we might, on the basis of our theory, have expected the river name to have contained the *ar(a)*, if not the *vara/vera*, element. In actual fact it has been thought to contain the element *gar-*, otherwise *kar-*, meaning a 'rock' or 'stone' but thought to feature in river names like the 'Garunna': 'Garonne'. This is another very common and early toponymical element: it is, in fact, one of the most strongly attested as pre-Indo-European.[13]

At this point we need to return to our *ara-* hydronym and admit that we have not yet attempted a full analysis of the knotty problems surrounding its origin. While Dauzat and others have seen it as pre-Indo-European, Nicolaisen and others follow Krahe in deriving it from an Indo-European root ***er-/*or-***, 'to set in motion'. Krahe thought that this and quite a number of related river names that have a wide distribution throughout Europe, represented an earlier

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[13] Alessio 1935; Fouché 1939: for k > g , p. 305, for g > *j(arem)* p. 306; Dauzat 1960: 81-90, 141; 1978: 49-50; Hubschmid 1959; 1953: 108- 12; 1960: 36-7; Rivet 1979: 501; Agud 1989: IV,183-4 *(arri);* Flutre 1957:147-9. For the Garonne, my note 48.

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language spoken throughout the region which he identified as an early form of Indo-European which he called 'Old European'.[14] A problem with his theory (and others like it) is that such a large area of linguistic unity at such an early period would seem unrealistic (e.g. Beekes 1995: 31-2). But as Nicolaisen (1982: 13948) has pointed out, these names need not indicate a shared language, a complete lexicon, but only a shared 'onmasticon', or more specifically, 'hydronymicon'. It might be that the factors that differentiate the hydronymicon from the rest of the lexicon are also those that cause it to show such apparent consistency over a wide area.

Krahe's river names clearly reflect to some degree the language group – Indo-European – of the peoples we now find using them, even if we follow him in recognising that they often reflect an early, or 'archaic', form of Indo-European. Thus the suffixing system can probably be in large part attributed to Indo-European speakers.[15] However, other characteristics have plausibly been attributed to 'substrate' language groups such as the typical 'a' vocalisation[16]: untypical of Indo-European but often said to typify the substrate. There is little doubt that a crucial difference between the 'hydronymicon' and most of the rest of the lexicon is that river names reflect earlier linguistic (sub)strata to a far greater degree. But that need not mean they (corporately OR individually) represent simply one single ancient linguistic stratum. Since they tend to outlast linguistic succession they might, to a greater degree, reflect the interaction between successive languages: especially perhaps the selective pressures of assimilation into different languages. Elements shared by different languages – or anything that constituted a

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[14] Dauzat 1960: 141 (cf. Rivet 1978: 389) and Rostaing 1950: 56 versus Krahe 1953; 1962: 314-19; 1964: 45-7 and Nicolaisen 1957: 224-5, 228-31; 1976: 182-3, 187, who gives strong arguments for seeing *Ar(a)-* river names as (anyway) pre-Celtic. For the root **er-l*or-* Pokorny 1959: 326-32; cf. Ekwall 1928: 17, 306.

[15] Kitson 1996, 81-4; contra Venneman 1994, whose analysis is flawed: so Trask 1997: 365-7. But our (non-Indo-European) root *ara-* also serves as a suffix according to Dauzat (convincing in the case of 'Is-ara', 1960: 135). Krahe sees an Indo-European *-r-* suffix; Hubschmid 1960: 56, 63, 97 has a 'mediterranean' suffix (not purely hydronymic) *-ara.*

[16] Venneman 1994, 248-52; Dauzat 1960: 79. Kitson 1996: 107, 110-13 argues strongly against this but seems to need to try too hard. See further my note 19.

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meaningful correspondence – might have been selectively favoured.[17]

For every hydronym one needs to understand not only the linguistic affiliation that accounts for the precise form we find but also what accounts for the adoption of that particular form in that particular local context. While the first may often lead us in the direction of Indo-European the second may lead us in the direction of the substrate. Since the 'meaningful correspondence' referred to above may often have been fortuitous phonetic correspondence then 'assimilation' – or 'secondary reinterpretation', or 'reinterpretation by popular folk etymology' – may have been far more widespread than is readily detectable[18] (who would doubt that 'York' was not simply from Anglo-Saxon *eofor* if there were no Roman records?). The point is that if this kind of thing happened not just occasionally but with some degree of consistency then it might explain the apparent uniformity of many river names over a wide area: without having to assume they are all the product of a single linguistic era (corporately or individually since the root, for instance, need not be the same age as the suffix).

On this general basis both of the explanations for *ara-* given above could be, in part, true (though to me the *er-/or-* root smacks of being suspiciously convenient for hydro-etymologists in need of a widely serviceable hydro-etymon) if it represents reinterpretation by Indo-Europeans of a pre-Indo-European name. Krahe, himself, (1953: 108) seems to admit such a possibility and in his later lists leaves 'Arar' out perhaps because it looks like a 'redoublement' untypical of his hydronomy as a whole (Kitson 1996: 86). Likewise the widespread hydronym in *kar-* might have been understood by Indo-Europeans in

[17] If these languages were genetically related then the elements in common might often have been the most archaic. The 'hydronymicon' might then have come to reflect - in some rather distorted way - an ancient linguistic unity: that had once existed, and had been forged, within some much more limited geographical area.

[18] Cf. my notes *21, 27, 29, 48* and discussion of the Rhine. This kind of thing is admirably illustrated by Kitson: *77-81* in a context of Celtic and earlier Indo-European.

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terms of their word *kar,* for 'hard',[19] even if it had represented, originally, pre-Indo-European *Kar,* 'a rock' (with which – as a voiced variant – we have associated our *gar >jar(em)* river name). In both cases the fact that the word had a point of reference in both languages could have been a factor in its adoption and preservation. The implication, here, is that many hydronyms, and other toponyms will have their ULTIMATE roots in the substrate. The 'Proto-Indo-Europeans', themselves, may have had an 'archaic hydronymicon' that was closer, genetically, to the hydronymicons of the peoples who they were to conquer, than was the rest of their language.[20] In any case they may have merely (over time) Indo-Europeanised an already existent 'system' – or set of names that were shared by different linguistic groupings. That will have come into being on the lines suggested above, through a series of linguistic successions: of which the advent of Indo-European will only have been one.

Another dimension to this 'system' might have been that it correlated with a degree of cultural unity perceptible within the European Neolithic and/or Bronze Age: perhaps especially in terms of religious belief on the theory that, in early (animist ?) times, hydronyms will also, necessarily, have been theonyms. One could see their archaic nature as something that lent them the authority of ancient tradition that helped to validate them as theonyms – and their theonymic value, in turn, as something that encouraged the retention of archaic forms. There is only one securely reconstructed Proto-Indo-European divine name (Beekes 1995, 39-40), the one we have referred to above, *Dyews,* and that may well have described an aspect typical of other deities ('shining, celestial') before it combined with *pater* etc.. (describing a male and ancestral aspect?) and ultimately crystalised into a separately defined deity as such.

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[19] Pokorny *1959: 531-2;* Krahe *1964: 58-9;* Nicolaisen *1957: 247-50; 1976: 187-8.* Contrast Kitson *1996: 98-100,* but here parallel arguments might apply: early words for 'cut' were often derived from words for 'stone'. In this case our Indo-European and pre-Indo-European forms might credibly have an ultimate genetic link: cf. my note *16.*

[20] For the theory that Indo-European derived from a language with 'a' vocalisation - or a/ə ablaut (cf. above with note 15), Bombard 1994: 73- 83.

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In fact scholars familiar with the Mediterranean regions have long tended to interpret toponymic roots in terms of a substrate language and some of them have suggested that this so-called 'Mediterranean' language (more realistically, group of languages) actually, at one time, covered most of Europe (e,g. Fouché 1939: 344, note 2). Thus the 'Dictionnaire' of Dauzat (1978), Rostaing and Deslandes attributes a pre-Indo-European origin to many toponyms which Krahe and his followers would categorise as Indo-European. The former scholars, and the Italians, Alessio and Bertoldi, who they were, in large part, following, have been criticised for having a far too cavalier attitude to the identification of pre-Indo-European roots: by Johannes Hubschmid, for instance, whose view of the *kar(r)* root, was far more narrowly circumscribed (e.g. 1963: 11, 98-9). However, though their conclusions may often rest on too little evidence (particularly in the case of roots to which no meaning could be assigned) to be reliably proven, it is another question as to whether their conclusions as regards the scale of the pre-Indo-European element in the toponymy were broadly correct. If recent assessments of the influence of the substrate in other fields are correct (e.g. Hamp 1990; Polomé 1990) then it is inconceivable that it is not also significantly present in the toponymy, which so typically reflects earlier linguistic strata. With the sharpening of focus of Hubschmid and others comes, arguably, an inevitable shortening of focus as regards the more remote origins and associations of words and toponyms. In sticking closely to the evidence we have to remember that there is an inherent bias in the evidence available to us: the vast bulk of comparative evidence for the determination of etymological roots is Indo-European.

At the moment, that gap in the evidence can only be filled by informed speculation so it might be wise not to dismiss too hastily the conjectures of, for instance, M. P. Fouché in his theoretical sketch of the kind of phonetic changes that might account for the relationship between substrate toponyms. One of his suggestions was the evolution of *ar-*, via *khar-* and *har-* from *kar-* thus explaining names like the Mont d'Ar and Mont d'Areng, in the Pyrenees, as from *kar-* – or its voiced derivative *gar-* (compare the 'pics du Gar') – 'a rock,

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mountain'.[21] It is significant, in this context, that it is precisely this kind of sound-change that has been hypothesised for our one surviving 'pre-Indo-European' language, Basque, in its early, 'pre-Aquitanian', stage.[22] Meanwhile, we might allow, as these scholars typically do (Dauzat 1960: 141; Rostaing 1950: 29-30), for a link between oronyms and hydronyms: from 'rock' to 'rocky stream' to 'stream' etc.. (except that we might also see a progression from 'god-of-mountain' to 'god-of-stream'). This would then help us with the problem we started with here because it might allow some link between our *Ara-r* and *gar* (behind *Jarem)* hydronyms: which might make it credible that they were thought of as signifying the same deity at some stage. Likewise Rostaing, typically, has a very different perspective on the *Var-* hydronym to Krahe's: he links it with *Var-* oronyms and derives it from a word meaning 'rock'.[23] If we have two separate roots here then we also very likely have a degree of assimilation of the substrate root to the Indo-European one. Following Fouché, anyway, we could hypothetically derive the former from *Kar* (= 'a rock') via a process similar to that which yielded *ar-* but with labialisation (Fouché 1939: 305-6, 313-4): this would allow us to see these two hydronyms (and the deities-become-saints they

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[21] Fouché 1939:309-11, 314-6; cf Rostaing 1950:21-6; Dauzat 1960: 83; 1978:49-50, 125-6 and 220-1, 107. Rostaing, p. 56, suggests a distribution of the *Ar-* oronym across the Mediterranean to Mount Ararat: others would insist we need ascertainable semantic as well as phonetic correspondence to be sure we have the same root.

[22] Trask 1997: 180-3 (and further 128, 130, 157-63, 178-9, 331-2, 398-403). Hubschmid denies such a sound change outside Basque but he cannot prove his negative and 'Aquitanian' (which might once, itself, have extended much further) may well have been one of a family of languages in which analogous sound changes occurred. Many Pyrenean oronyms seem to have to do with Basque (h)arri,'a rock' (Fouché: 309-11, 314-6): we would need to assume a very ancient kar- (or ultimately Kai-, Dauzat 1960: 74, 91-8; Fouché: passim) ancestral to karr- (h)arri (Dauzat 1960: 83) as well as various oronyms in kgar- and ar-, some of which might typically have been reinterpreted in terms of more recent words - themselves also ultimately derivative of kar- - in both substrate-derived Romance and Basque, like Karr-and its Basque equivalent (h)arri-. Also, in Basque, perhaps aitz, 'crag'; aritz and arte,types of oak; aran, 'a valley'; gara, height', etc.. See Agud 1989.

[23] Rostaing 1950: 2907-9; Rivet 1979: 486; Dauzat 1978: 230; Flutre 1957: 299-306.

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also signified ?) as closely related.[24]

At this point we can return to our immediate, early medieval, context. With respect to the hypothesized influence of the association, deriving from pre-Christian traditions, on the revisers of the *Martyrologium* at Auxerre, we can take note of the Insula *ARAlen(en)sis in* Tirechan's late seventh century Life of St. Patrick (Bieler 1971: 125, cf. the *Tripartite Life*, 36) which René Louis has identified with an 'Orgelanum' recorded in 1229 at Auxerre (Louis 1951). This suggests that the toponym, *ara-*, at least, was found at Auxerre, while the cult of St. Julian of Brioude was certainly well established there by the sixth century (Picard 1992: 64-5). Of course we might suspect the island-name actually contains a theonym (we will suggest parallels below) related to the 'Arus' of the Segusiavi. The possibility exists that this divine name might bear some relationship to some of the various place, personal and tribal names including *Ar-* found throughout the Celtic regions, and perhaps beyond.[25]

We will obviously be most concerned, though, to find some sign of any pagan precursors to our Christian cult figures in Britain. The *ara* river-name is certainly found in Britain. If we interpret the *-ara*

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[24] If we attribute to Kar,'a rock' an original theonymic value then we could interpret it (or an original Kal ?) as having originally signified something along the lines of 'mother earth'. This might allow it a central place in Neolithic culture such as to make a large number of derivatives credible: the root is remarkable in terms of the large number of very divergent languages, and even language-families, in which it is found. Hubschmid suggests widespread borrowing but this does not really answer the question (which, surely, we should be asking) of why it is that certain roots seem to survive much longer and (as a factor of this?) appear in more divergent languages than the vast majority (giving the impression to some of a single ancestral language of very wide distribution). Our approach offers a theoretical explanation because we have suggested that elements of the 'onmasticon' tend to outlast linguistic succession and so within a group of interacting languages their rate of mutation overall (affected by cross-language assimilation and a resultant archaising tendency as outlined above) might operate on a rather different – a bigger – time-scale. We need to recognise that some ordinary words derive from proper nouns, rather than vice-versa: those of the former which contain an exceptionally longlasting and widespread root might represent 'spin-offs' from a name (on our theory, ultimately a 'theonym') whose cultural importance, or prestige, has ensured it an exceptionally long and 'productive' life. Cf. my note 21.

[25] In Holder 1896. Bromwich 1978: 277 suggests some of these (I, 171; III, 649-50) might be related to the British mountain name, 'Aran', and 'Aranrot', see below.

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suffix as containing the hydronym, this includes South Wales where 'Loughor' is probably from 'Leucara'.[26] Otherwise *Ar* on its own is probably found in the 'Ayr' ('Ar' in 1177) of South West Scotland and the 'Oare' of Somerset ('Ar' in 1279) while there is a variety of suffixed forms elsewhere.[27] Matching the mountain names in *Ar-* from the continent is the Welsh mountain name, 'Aran', also found in the name of the Scottish Isle of Arran, which has been associated with oronyms from the Mediterranean like the mount 'Arana' of Sardinia and also with Basque *aran*, 'a valley', which, in turn, has been associated with the hydronym *ara*.[28] That there might, indeed, be some kind of association between our hydronyms and oronyms in *Ar-* is suggested by the location of the isle of Arran directly opposite the mouth of the Ayr (earlier 'Ar'). If these names had theonymic value it might help to explain the semantic jump, 'rock' or 'mountain' or 'island' to 'river' or 'valley' etc... One might compare the river 'Garrunna' (Garonne) near the 'pics du Gar': it has been suggested the former takes its name from the latter but it might be that both have reference to a divinity, locally important at some stage. Flutre (1957: 211-5, contra Dauzat) suggests the '-unna' in 'Garunna' is a variant of the river/divine name suffix *-ona*. The comparison is doubly relevant because in Roman times a 'Vindo-GARA' gave its name to the sea between the Ayr river-mouth and the isle of Aran (Rivet 1979: 501-2), while in the Pyrenees the Garonne flows through the Val d'Aran. In both cases we seem to have a close relationship

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[26] Rivet 1979: 388-9 and cf., here, 'Lutudarum', 'Nabarus', 'Tamarus', 'Metaris', 'Varar'. For 'Aire' ? < 'Isara', Ekwal 1928, 1-3. For ara as a suffix, my note 14.

[27] Ayr and Oare: Ekwal 1928: 305-6; Nicolaisen 1976: 187 1957: 228-9. Other derivatives: Ekwal: 16-7, 139; Nicolaisen 1957: 229-31, 1976: 187.

[28] Pokorny 1933; Agud 1989: 111, 900-1 (with further refs.); Dauzat 1960: 132; Bromwich: 1978: 277; Watson 1926: 87, 97. hi mid Wales are four hills 'Aran', or 'Arenig', and an 'Afon Aran'. Watson notes the Aran isles off Ireland have a different Gaelic name to the Scottish Arran - 'Aru' as against 'Arainn' (earlier 'Ara' or 'Arand/Arann') – and suggests the former is from Irish aru, 'kidney', but we may have a very early name (typical for islands) that has evolved differently (e.g. in one case with a P-Celtic stage, or in another with assimilation to a similar word ?). Ifor Williams 1945: 17 notes that ar < ardd, 'hill, high', as in 'Bennar' < 'Bennardd', is not so likely in 'Aran'. Nicolaisen 1996: 4 classes 'An-an' as one of a group of Scottish names likely to be pre-Indo-European. For the pre-Indo-European substrate in the British isles see, e.g. Wagner 1987; Schmidt 1990; and for its likely late survival in Scotland, Jackson 1955.

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between names in *Ar-* and *Gar-* within a localised context: that might support the link between the two that we suggested above (see further Dauzat 1960: 83, 90, 140-1; Flutre 1957: 147-9 and my note 48) That there was indeed a theonym related to the island/mountain name 'Ar(r)an', meanwhile, I shall seek to demonstrate below.

To return to our other cult name, we have noted already – the goddess 'Sulis' at Bath, while, as in Gaul, there was also a cult of the 'Suleviae.[29] There may have been, then, parallel cult traditions to those we have suggested in Gaul so that a cult introduced from Gaul, presumably in Christianised form, will have had some local 'resonance'. We can easily imagine Germanus playing some parallel role in the Christianisation or regularisation of such a cult as he seemed to have with others in Gaul. We should remember that his primary mission was 'antiheretical' and it was in this context (Arian rather than Pelagian) that Ambrose had demonstrated the efficacy of the sudden discovery of martyr's graves and the creation of new martyrs' cults (*Augustine's Confessions* 9.7; Delehaye 1930: 75-8). Might it be that sometimes Christians were prepared to make certain compromises, to gain the popular support they needed, to combat what they felt to be that greatest of evils, the perversion of their faith by 'heresy' ? Germanus, anyway, certainly did display an interest in the other British martyr, mentioned by Gildas, 'Alban' (Constantius's *Vita Germani* 16). Clearly, as regards the Gaulish and British manifestations of our hypothesised *Ar/Ver* + *Iul/Sul* combination, we cannot be sure to what extent we might be dealing with independent parallel phenomena, or how much mutual influence there might have been and which direction it went in: but nevertheless the influence of Germanus provides us with a possible explanation for the kind of rather close parallelism we have suggested between the 'dual' cults of Iulius and Aaron, and Julian and Ferreolus, respectively.

We need to bear in mind, of course, the specific location of the cult at Caerleon (the best candidate for the *Legionum Urbis* of Gildas) and its likely association with the local tribe, or *civitas,* of the Silures. On this basis we might turn our attention, as we did in France, to the major river of the area. The name 'Sabrina' (for the

[29] To her association with thermal waters cf. that of our Arus/Hercules. On the other side of the Arus coin appeared an image of 'Mnerve Segusia': cf. 'Sulis Mnerva'.

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Severn) - does not at first sight seem helpful to us. However, it may have had a variant, or earlier, name (as 'Sauconna', for the Saone, was preceded by 'Arar'), while there might be a chance that 'Sabrina' actually does incorporate our *ara* hydronym. Rivers with what is probably the same initial *Sab-* element in Gaul are mainly of the type *Sab-ara* and, in fact, hydronyms of the 'Sabara/Savara/Samara' type were very common there.[30] There seems a good chance, then, that 'Sabrina' represents the same type but with the added suffix *-ina* (as in 'Lind-inis'), and so we might see it as representing *sab* + *ara* + *ina*. It is entirely credible that this dominant natural feature, with its spectacular 'bore' and which seems to have been associated with an important Roman-era cult, based on its banks at Lydney,[31] might have anciently had some special religious significance, for the area: perhaps parallel to that of the Rhone in Southern Gaul. If it was in any way associated with the hydronym, *'Ara-'* (once meaning literally 'water, river' ?) then we have some kind of theoretical basis for a local cult of 'Aaron', as derived from *Ar-on(a)*, or similar, to match our hypothesised derivation of 'Ferreolus' from a cult figure associated with the Rhone.

Given that the Severn and its estuary also constitute one of the major geographical features of the island as a whole we can begin to envisage viable hypotheses for why – judging from Gildas alone (as

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[30] E.g. the 'Sevre' < 'Savara' and 'Sambre' < 'Samara'. For Dauzat, et al, *Sav* = *Sab* (on the basis of e.g. 'Save' and 'Sambre' < 'Sabis'), while *Sam* appears also to be a variant of *Sab/Sav*. See Dauzat: 1978: 2, especially note 7, and 81-3 which lists 13 certain or probable e.g.s of *Sab/v/m* + *ara* (including two *Savara > Sevron). Cf Dauzat: 1960: 138; Rostaing 1950: 243; Rivet 1979: 450-1. Contrast Pokorny 1940: 79, 121 who suggests an 'adjectival *sabro*' < IndoEuropean *sab*, separate from *sav* < *sow* (cf. Krahe 1964: 50). For another theory Hamp, *Studia Celtica* 22/23, 1964, p. 50. It may be that we have an original pre-Indo-European form (perhaps suffixed: for *Sam-p, Sam-b* Hubschmid 1960: 56-8) that has undergone (sometimes temporary ?) assimilation to (two different ?) Indo-European river/water words. For *ara* as a suffix, my note 14. It might be possible the -rin element represents a pre-Celtic vocalisation of *rei-n* (from *(a)ra* + *-n* by assimilation ?): see my discussion on the Rhine.

[31] Both the name of the god, 'Nodons'(='Cloud-maker', O'Rahilly 1946:495) and the cult itself demonstrate our sun/water duality: Green 1986: 159-6 1; Ross 1992: 47-8, 230-3.

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we must) – the cult of Aaron and Iulius appeared to be the second most important one, after that of Alban, in sub-Roman Britain, if this had nothing to do with the prestige of actual martyrs so named. Also relevant in this respect is the fact that 'Aquae Sulis' was arguably the most important cult centre of Roman Britain. On the basis of the arguments given above we can see why 'Julius' might have been the most obvious name for the Gaulish Germanus to substitute for 'Sulis', in any kind of Christianised successor to that cult: the attraction of whose adherents might have helped him in his fight against the Pelagian 'heretics'. The fact that by the time of Gildas the martyrs were thought of as having been from Caerleon need not mean anything more than that their cult centre was at or near Caerleon in his time : in fact had the cult, or even the supposed relics, of the martyrs originally come from elsewhere a process whereby they became 'localised' at Caerleon would not be without precedent in the better records that we have from other parts of the ex-Empire (Van Dam 1988: 80-1, 87)); although we can also envisage a process whereby some kind of (Christianised) local 'parallel' to the cult (at one time) at Aquae Sulis might have gained prestige, or broad appeal, from its associations with it.[32]

If the cult was especially associated with the Silures then its evident dominance in sub-Roman times might reflect the dominance of the Silures themselves, at some important stage in post-Roman history: this would help to explain why this important cult was based on Silurian territory and not at Aquae Sulis. Such a dominance the Silurians could, conceivably, have gained from a leading role - parallel

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[32] In fact the cult known to Gildas may have been near, rather than at, Caerleon: nearby 'Basaleg' (< *Basilica)* has a name unique in Wales but in Ireland it was especially associated with a church that held important relics (C. Doherty, "The Basilica in Early Ireland", *Peritia* 3, 1984: 303-13). One would have to assume the cult at Caerleon (Bulmore ?) was set up later, inspired by the mention in Gildas, and that the association of Basaleg with St Gladwys (demonstrated by Knight 1993: 9-10) was not an ancient one: that there was, in effect, a break in, or considerable distortion of, the tradition from the sixth century. If the river that passes by Basaleg, the Ebbw, earlier, e.g., 'Ebbot' (Rhys 1893: 284) is related to Welsh *ebol,* 'a colt', it may have had associations in pagan solar religion: for the horse and the sun's journey across the sky, e.g., O'Rahilly: 290-4.

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to the one they played some centuries earlier, against the Romans - in stemming the Saxon advance across Southern England, around the late fifth century. It is clear that by the time of Gildas the centre of power had moved deeper into the 'highland zone' to Maelgwyn's Gwynedd but if there is any geographical basis behind his list of reprehensible British potentates then the mysterious 'Aurelius Caninus' occurs in just the right place (*De Excidio* 30) to be a king of 'Gwent', the kingdom that evolved out of the civitas of the Silures. The fact that his name does not occur amongst those of the legendary kings of Gwent need not be significant if those are entirely fictional, anyway. Late legends (*Historia Britonum* 41) about Gildas's hero of the wars against the Saxons, Ambrosius Aurelianus, associate him with South East Wales, while his name has been seen as finding a faint echo in our AURELIUS Caninus.

We will find, however, the most crucial support for our hypothesis in the medieval secular literature in which we find many figures who have their origin in pagan mythology, and where we might, indeed, expect to find some trace of our cult figures with pre-Christian antecedents given that they seem to have achieved some kind of significant dominance, at least at the time of Gildas. We do have a fairly obvious candidate to represent such a late, literary, derivative of, at any rate one of our two cult figures, in the form of the *Arawn urenhin Annwuyn* – 'Arawn, king of Annwn' – who appears in the Mabinogi tale, 'Pwyll, Prince of Dyfed' (Thomson *1986: 2;* Jones G&T: *1949: 4).* The name, otherwise difficult to account for, represents a possible development from an earlier 'Aaron' (Jackson *1953: 307):* the medieval version of the Biblical name was 'Aron', but that will clearly have been influenced by continuous contact with scriptural tradition, while 'Arawn' might represent an independent development in an originally purely oral context: the two names evidently were regarded as equivalent in medieval times (Bromwich *1978: 273).* Even a Christianised figure (who may always have been understood differently by different elements of society) may have entered the folklore and ultimately the literary tradition in the way that many pagan figures did, while there may have been some link between the martyr's association with death and 'Annwn', (the 'otherworld') as 'world of the dead'. We do not, of course, find 'Araun' associated with a 'Iwl' (or a 'Sul'), but we do find him closely

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associated with a 'Pwyll', who, like him, has no obvious antecedent in pagan mythology. In fact the 'P-' of this name, 'Pwyll', as well as its literal meaning, is to be seen in the light of its close relationship with another name 'Pryderi': these two alliterative names with related meaning have developed together in an oral context and it is possible that the 'P' of Pwyll originated in a process that adapted an earlier name to one that paired appropriately with 'Pryderi'.[33] We have to bear in mind that some features of the 'Mabinogi' might go back, not to the original 'pagan mythology' of the Celts, but rather a version of it influenced by the syncretism of the post Roman period.

Another figure we need to consider is the 'Aranrot', who appears in 'Math, son of Mathonwy' (Williams 1951: 67- 92; Jones G&T 1949: 54-8): while 'Arawn' might represent a derivative of our Christianised, or syncretic, cult figure, the 'Ar-an' element of 'Aranrot' might represent an inheritance direct from our suggested pagan antecedent for that figure. Rachel Bromwich has suggested a link with the name 'Aran', used for mountains in Wales and an island off South West Scotland, that we have already suggested, above, might be etymologically linked with our 'Ar' hydronym, appropriate to the river 'Ayr', on the mainland opposite. The 'rot' part of the name, 'Aranrot' probably means 'wheel' (Bromwich 1978: 27-8: one might think of the characteristic Celtic, and pre-Celtic solar symbol, see O'Rahilly 1946: 304) but it seems likely that the full name, 'Aran-rot', derives from an earlier version of 'Caer Aranrot', the fortress closely associated with her in the Mabinogi tale. 'Rot' will then have been reached by assimilation from an original 'rat', equivalent to the Irish *rath*, meaning 'mound' or 'fort', and the 'Caer' will have been a subsequent tautology. W.J. Gruffydd (1928: 187 ff.) used Gaulish 'ArgenroRATum' ('Strasbourg') for comparison, as this corresponds closely to 'Aryanrot', a common variant form for our figure: both would then have meant literally 'Silver fortress', although in one case

[33] For pwyll = 'sense, wisdom', see Thomson 1986: 24; Bromwich 1978: 497. Grufrydd 1953: 35 ff. suggests the relationship between the two was not originally the one that appears in the extant tale. 'Pwyll', anyway, was most likely famous as a 'king of Annwn' (cf Bromwich 1978: triad no. 26) in the same way that 'Araun' appears to be. For Pwyll and Pryderi (= 'care', originally 'loss') as a pairing of the type Dylan eil Ton ('Sea son of Wave') etc.. see Haycock 1983: 66, note 4; Bromwich: 496-7; Thomson: 41.

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the meaning will have changed to 'silver wheel'.

Rachel Bromwich, however, has argued persuasively that 'Aran' is primary to 'Aryan': it is important to note, then, how easily 'Aran' could become assimilated to *aryan*, 'silver'. We might see this in terms of our typical assimilation of pre-Indo-European to Indo-European names and consider whether other names with the element *argan/aryan* might share this ultimate derivation: like 'Argent-oratum' itself. This city was on an island between two channels of the river 'Ill', earlier 'Elellus', but another channel of that river bears the name 'Aar', and if this represents an earlier or alternative name of the river it might explain the existence of an 'Argentovaria', on its banks further upstream, and an 'Argentaria' in the region of its headwaters.[34]

Then there is the common river and stream name, 'Aryanell' (Padel 1985: 11). It is significant in this case that there was evidently a cult-figure – the virgin 'Arganhell', associated with St. Dubricius in the Book of Llandaff (Rhys 1893: 82-3; Doble 1971: 75-6) – whose name derives from the stream-name in a way that parallels our hypothesised derivation of Aaron from the hydronym *ara-*. More relevant again is the fact that the god Nodons associated with the temple complex at Lydney has been identified with the 'Nuadu' of later Irish literature, whose epithet was *Argatlam* meaning 'of the silver hand' or 'arm' (O'Rahilly 1946: 496; Bromwich 1978: 428). This epithet has been thought of as having reference to the Severn, perceived as an 'arm' of silvery water (Ross 1992: 2303): this might seem all the more likely if the Severn was sometimes referred to by a name 'Ara-', or a form of this with a suffix, 'Aran', that could easily become assimilated to the word *argant-*, for 'silver'. Alternatively the epithet might have evolved out of a direct association between Nodons and a related, or equivalent, cult-figure 'Aran(-)' .

That such a figure existed seems likely. 'Caer Aranrot' was the name of "a rock visible at low tide about a mile seawards of 'Dinas Dinlle' on the Caernarvonshire coast" (Bromwich 1978: 278). This, together with the name of one of her two sons, 'Dylan eil Ton', 'Sea

[34] River-names in Arg-,in the West, are typically associated with the Celts (Kitson 1996: 83-5) and might represent their typical re-interpretation of earlier names in ara- (or earlier Indo-Europeanised ara-nt).

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son of wave', would seem to support the idea of 'Aran(rot)'s association with water, the sea, and especially the island name 'Ar(r)an' (cf. Gruffydd: 1928: 210 ff.). The Scottish Isle of Arran was associated with the 'Otherworld' in Irish legend (Watson 1926: 97) – and with the god 'Manannan', whose name was linked closely to the island name 'Manau', for the Isle of Man, and probably also with 'Mona', for Anglesey.[35] It looks as if originally there was a parallel divine name, linked to the island name, 'Ar(r)an', of which 'Aran(rot)' is a late derivative in folk tale and legend. As 'Manannan' presides over the 'Otherworld island' so Aranrot, as Gruffydd (1928: 187-96) points out, was perceived as 'Queen of the Otherworld': and we can compare 'Arawn' as king.

It might also be significant in this respect that the Breton St. Aaron was associated with an island of St. Aaron (now of St. Malo): the fact that his chapel is on the highest point of that island might further suggest some relationship of the saint's name to the oronym. We might suspect, then, that the saint's name here, represents a Christianised version of our hypothesised divine name: perhaps under the influence of the name of our British martyr (derived in a parallel way ?) with whom this 'Aaron' may have been equated at some stage.[36] Finally, with Caer Aranrot, the Scottish isle of Arran, and the isle of St. Aaron, we can compare the *insula Aralensis* of Tirechan, mentioned above.

'Aranrot', meanwhile, in the mythological scheme of things, was the mother of 'Lleu Llaw Giffes', whose name features in the 'Dinas Din-lle', mentioned above.[37] 'Lleu', the equivalent of Irish 'Lugh', is derived from earlier Celtic 'Lugus': the name of a prominent Celtic deity, whose name means literally 'the bright one', or the like, and who has been interpreted on this basis as, in origin, a Celtic 'solar

[35] Bromwich 1978: 441-3; Rivet 1978: 410-11, 419-2; Rostaing 1950: 213.

[36] St. Malo, Aaron's successor, was said to have come from Gwent: Lot 1907: 295, 353.

[37] Williams 1951: 80; Jones G&T: 54, 56; Bromwich 1978: 420- 1: Llaw Giffes ='skillful hand,'; cf. Gruffydd: 1928:60 ff.

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hero' or 'sun-god'.[38] We might, then, see him as some kind of equivalent to our Iulius/Sulis, and see his close relationship to 'Aranrot' as mirroring, in some sense, the close relationship of our 'Iulius' and 'Aaron'. It is significant, in this respect, that 'Araun map Kynvarch', the only other 'Arawn' of Welsh tradition besides the one of the Mabinogi, was said to have had a brother 'Lleu' (the only other 'Lleu' of Welsh tradition). This is the case, both in the *Brut y Brenhinedd,* the Welsh version of Geoffrey of Monmouth's *Historia Regum,* in which 'Arawn' replaces Geoffrey's 'Auguselus' and 'Lleu', his 'Loth' and in an early 'englynion' in the thirteenth century 'Black Book of'[39] Carmarthen. It must be highly significant for our hypothesis, then, that there does indeed seem to be some kind of link between figures named 'Aran-rot' and 'Araun', and that that link took a form that could be seen as mirroring the primary association of our suggested 'syncretic cult-figure', Aaron.

To return to our Christianised cult of late antiquity: our theory about it might have significant implications, of a more general kind, about the relationship between Christianity and native pagan religions. It might suggest some kind of merger or alliance between the new faith Christianity and the more ancient religious traditions of Gaul and Britain – even, perhaps, involving some kind of revival of the latter. It might be relevant that the cult of Julian was a notably rural cult, outside the main civic and episcopal centres while the cult of Ferreolus seemed to have been something Mamertus felt he had to bring under closer control (*VJ* 2; Van Dam 1993: 165, 44)). Then again, it is tempting to guess that our pair of martyrs (> ancient deities) had some connection with the pair of classical gods worshipped at Brioude: it is important, in this respect, that Mercury was probably identified with 'Lugus' (e.g. Bromwich 1978: 421), while

[38] Jackson 1953: 441; Evans 1967: 219, with more refs. Wagner 1987 (who would deny this etymology but nevertheless see Lugus as a "Celtic representative of the all-knowing sun-god": 1970: 23) would see Lugus as a deity borrowed from neighbouring peoples while the Celts were in the East: as such he would represent a third category of deity to the ones (original pan-Indo-European and native pre-Indo-European) mentioned above.

[39] Roberts 1971: 29, 30; cf Bromwich (1978: 422-3, 185: triad 70) who explains the 'Llywarch' in the englynion (Jarman 1982: 82) as a mistake for 'Cynvarch'.

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Mars was often associated with the healing cults of sacred waters.[40] It might be that we are dealing with a sub-stratum of very ancient religious traditions that had become assimilated to a Celtic and classical religious 'superstructure' in turn and were now being assimilated to the Christian. At any rate it might seem more credible that Christianity could have merged unobtrusively with such a 'sub-stratum', rather than directly with the more high profile classical and Celtic gods of the time. At the same time if a degree of imaginative and rather artificial 're-creation' of old religions was involved, at any stage, this might not have been unusual for late antiquity, either (cf. the pagan revival of Julian): especially in relation to cults involving 'Sol', 'the sun', which also came closer than any other pagan deity, towards some kind of syncretic merger, with the Christian one.

On this basis the derivation of our British and Gaulish martyrs from pagan cult figures is likely to have been part of a wider phenomenon: what follows is a brief survey of analogous cults – more, or less, closely related – that might help to both demonstrate that and reinforce our central hypothesis. First of all we can note that the cult of Julian became fairly widespread rather rapidly[41] Which might seem surprising for a localised cult of the Auvergne countryside unless there was something about it that gave it some special resonance in other places as well: but if so we might expect that in some cases we might find parallel but apparently independent cults (like that of the British Julius, on our hypothesis). In this respect we might note the Julian, legendary first Bishop of Le Mans, who was "called the apostle of Celtic Gaul", but for whom we have no evidence prior[42] to the seventh century. Even if a real bishop of that name

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[40] As at Lydney where he was identified with Nodons, and Bath: Ross 1992: 228-30.

[41] See *VJ* 32-34,41,47,50; Gregory's *Historia* 9.6; Delehaye 1933: 371. Van Dam 1993:43 notes that "unlike many other saints' cults, the cult....expanded apparently without local episcopal patronage", but also emphasises the role of Gregory of Tours' family.

[42] Attwater 1956: 1, 183; *ASS* Jan. III, 376-92; Duchesne 1907: II 309 ff. Pietri 1987: 41-56; Ledru 1913. A reference of c. 616 indicates a rather minor cult at that time (Ledru 56) when Victurius (late 5*th* c.) was the most famous bishop of Le Mans. Our cult might have been to a 4*th* c. bishop (Ledru. 131 ff., 145) but alternatively a deep-rooted local pagan cult figure might have been reinterpreted

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was involved we have to explain what was behind the rise to local dominance of this particular cult, which may have spawned, or was anyway confused with, the popular legend of another Saint Julian: 'the Hospitaller'. There are also suggestions that this latter Julian evolved out of the saint of Brioude (Gaiffier 1945) but there is no doubt that his legend is derivative of pagan solar myth.[43]

Then there is the Julian, associated with Saint Caesarius of Terracina, in Italy, whose church at the Imperial palace on the Palatine in Rome, recorded from the seventh century, shows that the name was interpreted in terms of the imperial title that originated with Julius Caesar.[44] Furthermore the hagiographical legend (*ASS* Nov. 1, 84-130) involves some of the same figures as the legend we have already encountered, recounted by Servius, associated with the origins of the 'Julian' gens: there is a 'Caesar(ius)', an 'Apollo' (the saint objects to the barbarous sacrifice made to this god), and, of course, a 'Juli(an)'. This strongly suggests the influence of the legends associated with the family of Julius Caesar, and is entirely compatible with the idea that a Christianised cult-figure Julius/an might derive from the same origin.

Finally, the manner of death of the two saints – sewn in a sack and thrown into the sea – brings to mind the legendary end of another saint Julian, the saint of Cilicia, of unverifiable origin, but who - for reasons "inexpliqué" - had an important cult and church at Antioch.[45] The *Hieronymianum* lists this saint on March 16th together with an 'Asclepiodot(us)' also from Cilicia (Delehaye 1931b: 147),

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as 'Christian ancestor-figure' or 'first bishop' of the city. By the 9*th* c. Julian had become a disciple of the Apostles sent by Pope Clement and his was the leading cult of the region.

[43] E.g. Saintyves 1907: 268-70. Cf the story also derivative of solar myth in which Lugh kills his grandfather, 'Balor' : O'Rahilly 1946: 313; Gruffydd 1928. The legend of Julian, of course, also associates him with a river.

[44] Delehaye 1933: 308, 409; Duchesne 1907: L 371, 377; John Diaconus' *Vita Gregorii* 20.

[45] Delehaye 1930: 200, 166. This Julian was sewn in a sack with 'reptiles' and thrown in the sea: the earliest mention is the panegyric by Chrysostom (Attwater 1, 604-5). CE the 'mythological' deaths in water of, e.g., Lugh in the Loch Lugborta, or Lleu Llaw Giffes whose grave lay 'under cover of the sea: Jarman 1982: 40.

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while later Greek sources name his mother as 'Asclepiodora': both these names are highly reminiscent of 'Asclepius'. Other Greek sources associate him with a Marcianus either as his 'persecutor' or as another saint of 'Aegea' in Cilicia, celebrated on the same day (*ASS* March 11, 415-8): this name brings to mind the 'Marsyas' associated in myth with Apollo (but whose name was also that of a river). There is the strong suggestion, then, of some kind of association between this 'saint' and a cult of Apollo.

Moving back to Gaul, Gregory of Tours mentions the saints of Cologne, known as the 'Aurei', so-called, he tells us, because of the glowing mosaics of their church (*De Gloria Martyrum* 61: Van Dam 1988: 85). He associates them with the legendary 'Theban Legion' (Attwater 1956: 111, 619-21), but as with many other martyrs so associated this is unlikely to have any basis in fact. The Bern text of the 'Martyrologium Hieronymianum' identifies the chief of these martyrs as 'Gereon' (Delehaye 1931b: 547-8; Duchesne 1894: 130).[46] There was, meanwhile, (as at Auxerre) a cult of St. Alban at Cologne, represented in a parish church dedication from the twelfth century as well as an 'Albinus', sometimes identified with him (Levison 1941: 338; *ASS* June V, 148-51). The cult, at Cologne, of Ursula and her numerous companions (Gould 1907: IV, 31247; Bromwich 1978: 3334) had its British connections, too. Fleuriot (1980: 134-58) has demonstrated that the British 'dispersion' went much further afield than just Brittany and British influence here (via the channel and the Rhine) – or some British connection – from an early date, prior to that of the Anglo-Saxon missionaries, is not impossible.

The most important question we have to ask, though, is whether Gregory's explanation for the 'Aurei' was really the right one – or the complete one: perhaps it actually only explains the final form of the martyrs' name, reached by assimilation. Relevant here is the fact that Louis derived the 'Orge-' of the 'Orgelanum' at Auxerre, from *Ara-*, via a "fausse Latinisation", 'Aurea-' (Louis 195 1), as well as the fact that the Rhine, like the Rhone, had tributaries with the name 'Ara',

[46] An alternative tradition about Cologne (Delehaye *1931: 557;* Duchesne *1894: 132)* involves 'Mauri' or African Moors who feature in later legends about Gereon (ASS. Oct.V, *14-60).* The name 'Mauri' is suggestive of a link with 'Aurei' (? < *ar-*): cf the one between 'Ger(eon)', *gar-*, and *ar-*, suggested below.

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– the modem 'Ahr', just south of Cologne, and the 'Aar', in Switzerland which is a major tributary, comparable to the Saone for the Rhone, and sometimes bore the same 'double' form of the name (Dauzat 1960: 132; 1978: 20). On the theory that many toponyms will have resulted from the interaction of Indo-European languages with the substrate the possibility exists that the Rhine, itself, might once have borne a related name. The current name, derives from an Indo-European **Rei-no* (Pokorny 1940: 55-8; Vendries 1959: R-27), but it may be that this name was itself attracted by an earlier, similar sounding, pre-Indo-European name, derived, along the lines suggested by Fouché (1939: 319-21), from the same pre-Indo-European element *Kar-* which, we suggested above, lies behind our *ar-* names. Thus *Kar* with suffix *-en* and loss of the *a,* would give *Krin,* which, via *Khrin,* and *Hrin,* would give *Rin:* parallel to the evolution *kar* > *khar* > *har* > *ar.* The element *kar-* would then represent the ultimate common origin of all these names: and via *gar-* we might perceive some link with 'Gereon', behind which Greek name, we might anyway suspect there stands one of an original pagan cult-figure, as certainly seems to be the case with the fabled Gereon of Greek legend, a figure significantly associated with the Western Mediterranean.[47]

In any case, further up the Rhine, at Mainz, where there was as at Cologne a cult of Alban,[48] from as early as 756, there was also the cult of a martyred bishop 'Aureus', recorded from the ninth century – but no earlier – in the martyrology of Raban Maur and whose relics

[47] His name has been linked to Celtic *garan,* 'a crane' but this seems unlikely to be the ultimate origin of a name that appears in an Italic context (Grant 1971: 53) as well as a Greek (Hesiod, Stesichorus). But a pre-Indo-European river/divine name in *Gar-* might have been reinterpreted in terms of this word. On this basis we might go some way towards reconciling the views of Hubschmid and Dauzat on the etymology of the Garonne *(Revue Internationale d'Onomastique* 7, 1955, 184-8, 282-4): an early 'Aquitanian' name in *Gar- (plus -onna* ?) might have been understood by succeeding Celts in terms of their word for crane - or a theonym based upon it (in *-ona ?*).

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were said to have been transferred to the church of St. Alban in 805.[49] This name 'Aureus' is altogether suspiciously reminiscent of the 'Aurei' while his association in hagiographical legends with a sister 'Justina', said to have been martyred with him, (a variant of the legend, from Heiligenstadt, has a deacon, Justinus') is distinctly reminiscent of the association of Aaron with Julius. We might guess that, comparable to the 'Ara' behind the *fausse Latinisation*, 'Aurea', there lies behind 'Iustinus/a' a name with the same *dyew-*, element, that we found in 'Iulius': specifically Old High German 'Ziu' (Simek 1993: 380: 'Zios-' in a compound like 'Ziostag') which we might feel was close enough to the late Vulgar Latin pronunciation, [dzu-], of Ju-stus, to become assimilated to it, in a context of the Christianisation of pagan cults.

The date for Aureus and Justina, in the martyrology of Raban Maur, was June 16th and on the same date in that martyrology is celebrated a 'Ferreolus', martyr of Besancon. Like the one of Vienne this Ferreolus is 'one of a pair', this time with a 'Ferrucio' - which looks like some kind of duplication of the name. The feast date of these two was actually September 5th in the Hieronymianum (conceivably influenced by the September date for the Vienne Ferreolus) but it was being celebrated on June 16th, probably at least as early as circa 700.[50] Finally, another interesting fact about the Cologne entries in the Hieronymianum is that on the same date (October 9th) as the mention of the 'Mauri' and Gereon with *aliorum* CCCXLII, in some manuscripts, there is mentioned a similarly large number of martyrs – though these are anonymous – from Vienne (Duchesne 1894: 132, Bern MS): there is the possibility, here, of some kind of significant association between the Cologne martyrs, or 'Aurei', with the home of the cult of our original Ferreolus, to match

[49] *ASS* June IV, 37-66 with 12th and 13th c. versions of the legend and a *Vetus Legenda* from Heiligenstadt, more than 100 miles to the North-East, which locates the martyrdom at Rustenfeld. Delehaye 1933: 359 quotes the 8th c. martyrology of Raban Mawr: in *civitate Moguntiaco passio sanctorum Aurel episcopi et Iustinae sororis eius, qui ab Hunnis vastantibus praedictam civitatem: for* the role of the Huns in the legend, deriving from the village of 'Hunum': *ASS* p.76.

[50] Attwater II, 552; Van Darn 1988: 93-4; ASS June IV, 4-13; Duchesne 1907:1, 48-61.

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that of the 'Aureus' of Mainz with the Ferreolus of Besancon. On our hypothesis this association would go back – ultimately – to the common origin of the hydro/theonyms, *ara* and *vara*, in *kar-*.

This is, I strongly suspect, to do no more than scratch the surface of a proliferation of related cults with shadowy pre- Christian antecedents whose connection with British cults might be more or less direct. This argues, of course, for a considerable degree of syncretism, especially in a British context, where our cult of 'Aaron and Iulius' must have been an important one and where many of the cults that emerge in much later evidence as those of the 'Celtic Saints' are of a very doubtful type that may well share a similar syncretic origin. One can compare the generally high survival of native culture in other fields (e.g. the language) compared to other parts of the ex-Empire and note the apparently thriving state of native pagan religion in later fourth century Britain (e.g. Salway 1981: 734). The unanimous literary testimony from the fifth and sixth centuries shows that Christianity was not only well established in (still Celtic) Britain but able to spread beyond the old Roman province: the likelihood is that some very considerable element of syncretism was the price that it paid for its rather sudden rise to complete dominance – as well as for the increasing role that it will have been called upon to play in providing that political and social cohesion, for which there will have been an urgent and overwhelming need in the absence of the Imperial army and administration.

At any rate the attempt has been made to show here one instance of this process of syncretism in action: and to provide a credible and coherent hypothesis for the origin of the cult of 'Aaron and Iulius' which does not involve the existence of actual martyrs, so named. Certainly if the evaluation of these martyrs as genuine is maintained then their significance is self-evident and limited. If it is not then we need to be alert to the fact because their cult may have important and hitherto overlooked implications, such as I hope to have thrown some light on here, in terms of some significant strands of continuity from the earliest strata of pagan belief through to the Christianised cults of the late and post-Roman periods and on into the Celtic literature of the Middle Ages.

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Crime as Destiny
A Study of Criminal Twins

Professor Dr. J. Lange

Departmental Director, Kaiser Wilhelm Institute

Translated by Charlotte Haldane

Foreword by J. B. S. Haldane

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